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E. L. Thorndike

VOL. 56, NO. 5 SEPTEMBER, 1949

THE PSYCHOLOGICAL REVIEW

EDWARD L THORNDIKE

1874–1949

Edward L. Thorndike died on August 9, 1949, three weeks before his seventy-fifth birthday and a half century, almost to the day, after his acceptance of a post in Teachers College, Columbia University, which he served continuously until his retirement at sixty-five, and in which he has held since then the title of Emeritus Professor.

Thorndike received the B.A. degrees from Wesleyan University (Connecticut), 1895, and from Harvard, 1896. Working under William James, he earned the M.A. degree at Harvard in 1897 and then transferred to Columbia to study with Cattell under whom he took the Ph.D. degree in 1898, offering as his dissertation *Animal Intelligence*, which has been regarded by many psychologists as marking the beginning of the scientific study of animal behavior. During the following year he taught psychology at Western Reserve University in Cleveland. On taking the post at Teachers College, Columbia University, in 1899 he launched his career in educational psychology.

At the time of Thorndike's death the staff in educational psychology at Teachers College were rounding out certain reports of his activities, especially those carried on since his retirement, to present to him on his seventy-fifth birthday. Among these papers is a bibliography of his publications to date. It contains 508 titles, almost exactly ten per year for the period of fifty-one years. The range of fields in which he worked is as unusual as the volume of his reports. He contributed comprehensive books or research reports or theoretical discussions, usually all of these, in almost every major area of psychology and education, and in many special fields such as lexicography and city planning.

In many of these areas Thorndike's contributions have become major considerations of scholars throughout the world. The following seem now to be regarded as of outstanding importance: his methods of studying animal learning; his "laws of learning"; his attacks on formal discipline and his theory of transfer of training; his introduction of statistical methods to education and educational psychology; his development of achievement tests and scales; his detailed methods and materials for the teaching of arithmetic, algebra, reading, spelling, and other school subjects; his work on group intelligence tests, especially those for adults; his theory of intelligence; his analysis of work and play; his concept of the "original nature of man"; his investigations of the nature-nurture problem; his defense of the idea that mental "qualitative" differences can be explained as "quantitative"; his studies of vocabulary; his new pattern for the

dictionary; his investigations of adult learning; and his theories of human nature in relation to society.

Thorndike's prodigiously vigorous work was guided by three major purposes: to demonstrate the law-abiding character of all human behavior, especially mental behavior; to show the unrivaled fruitfulness of applying the methods of science to the problems of the individual and society; and to launch within psychology the policy of employing patterns of quantitative research as exacting as those employed in the physical sciences. In the early years of his career, psychologists as well as educators, philosophers and religious leaders were shocked by his insistence that "anything that exists, exists in some amount and can be measured" and in the later years some representatives of all these groups misunderstood his conviction that quantitative increments can account for apparent or real qualitative differences. A basal principle in Thorndike's code as scientist was, for example, that the difference between a genius and an imbecile, and between insightful learning and trial-and-error learning, can be explained, for scientific purposes, more helpfully as quantitative than as qualitative differences. Accordingly he opposed all his life the tendency within the social sciences to accept such popular concepts as "reason," "insight," "intelligence," and other "mystic potencies" as sound or useful explanations of mental activity.

For today and tomorrow's students who wish to explore Thorndike's psychology with some thoroughness, the bibliography of his published works up to April 1940, published in the *Teachers College Record* of May 1940, will be brought up to date in the October 1949 issue of the same journal. Only a few days before his death Thorndike finished correcting page proofs of a moderately sized volume, *Selected Writings from a Connectionist's Psychology*, which consists of his own selections from his writings, a book which provides a convenient brief introduction to the character and range of his work.

Thorndike was a member of a large number of scientific and other scholarly societies, including the National Research Council, and served as President of the American Psychological Association, the American Association for the Advancement of Science, the New York Academy of Science, the Association of Adult Education, and others. He received honorary degrees from seven universities: Wesleyan, Columbia, Chicago, Athens (Greece), Harvard, Iowa, and Edinburgh. He received the Butler Medal in gold from Columbia in 1925 for "exceptionally distinguished contributions" to the social sciences. He was a member of the Century Club.

Thorndike received all his schooling except for the final year at Columbia in New England where his father was a Methodist minister. At the age of twenty-six he married Elizabeth Moulton, of Boston. His brother, Ashley, deceased, was Head of the Columbia University Department of English; his brother, Lynn, is a member of the Columbia Department of History, and his sister recently retired from teaching in a high school. His living descendants are a daughter, Mrs. Freeman Cope, who at one time taught mathematics at Vassar College; his sons, Edward, Professor of Physics, Queens College; Robert, Professor of Education, Department of Educational Psychology, Teachers College, and Alan, a Harvard Ph.D. in physics, now engaged in research in a government agency, and thirteen grandchildren.

A few words should be said about Thorndike as a man, especially for those whose contacts with him were casual. Physically he was often de-

scribed as a rugged "bear of a man." He was the friendliest of persons, generous and kind, even if quick to rebuke a wrong. Although his theories were the center of controversies all his life, he took criticism, even stupid abuse, with attentive good will, and his own criticisms were directed at issues and not at people. His buoyant good humor and sparkling wit made him a delightful companion, teacher, and public speaker. So great was his enjoyment of his work that he spent relatively little time, even after his retirement from Columbia, in conventional play. He often chuckled that "the college pays me for doing the very things I like most of all to do." As a young man, however, he relished his canny game of tennis, and later on an occasional game of bridge or a detective story, and those of us who have lived near him prize Thorndike, the genial country gentleman, as much as Thorndike, the psychologist.

ARTHUR I. GATES

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EMOTIONALITY AND PERCEPTUAL DEFENSE

BY ELLIOTT McGINNIES

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During the past decade, a number of experimental investigations have progressively revealed the so-called "dynamic," or motivational, aspects of perceptual behavior. No longer do we view perception as organized solely in terms of the structural characteristics of stimulus objects or the frequency with which the individual has been exposed to these objects. Perceptions are structured not only with respect to the limiting stimulus conditions, but also with regard to the possibilities of reward (11, 12), need fulfillment (1, 7), attitudinal orientation (10), potential anxiety (4), symbolic value (3), and release from tension (2), to mention just a few. In order to describe such facts as the perceptual selection and accentuation of valued objects and the elimination or distortion of inimical stimulus objects, it has been found convenient to invoke mechanisms of *sensitization, defense,* and *value resonance* (10), *vigilance* (2), and *primitivation* (4). Finally, playing host to these varied and intricate functions is the "ego," in whose service, presumably, the various perceptual adjustments operate.

It seems well established, then, that the perceptual "filtering" of visual stimuli serves, in many instances, to protect the observer as long as possible from an awareness of objects which have unpleasant emotional significance for him. Does this process, however, entirely insulate him from the emotion-provoking qualities of the stimulus situation? It is to this problem that the present discussion is addressed.

If we view emotion essentially as a motivating condition of the individual (6), the critical nature of the relationship between emotion and perception becomes apparent. Emotion does appear to represent a highly organized and directed state of the organism. Consequently emotion-inducing stimuli may be expected to initiate those perceptual responses which will be consistent with the general picture of emotional adaptation. Several exploratory investigations have indicated that the individual both perceives and reacts in a manner consistent with his emotional response to stimulation. That tension (defined as reactivity to threat, deprivation, or thwarting) will induce perceptual "accentuation" of objects previously associated with the anxiety-producing situation has been demonstrated by Bruner and Postman (2). More recently the same authors have shown that frustration, induced by sarcasm and criticism, will raise the perceptual thresholds of observers to tachistoscopically-presented words. When, on the other hand, individuals are faced with stimulus objects which are not actually threatening, but which represent for them areas of little interest or some antipathy, they also generally display raised thresholds of recognition (10). This process of perceptual "screening" apparently is acquired by the individual as a technique for organizing perceptions around value expectancies so as to produce maximum reinforcement of those expectancies.

One question intrudes repeatedly into interpretations of these experimental findings, namely: "How is a raised or lowered threshold of recognition for inimical stimulus objects accomplished before the observer discriminates them

and is thereby made aware of their threatening character?" While the answer to this question will follow eventually only from fuller knowledge of the neurophysiological processes underlying perceptual response, detection of any one aspect of physiological reaction accompanying perceptual behavior should throw some light upon the processes by which perceptual defense is effected. One might conjecture, for example, that stimuli of an appropriate sort will arouse autonomic reactions characteristic of anxiety or pleasure *prior* to conscious awareness of the nature of the stimulus. If this is the case, we might expect to find a change in galvanic skin response in reaction to visually presented stimuli with emotion-provoking connotations before the subject is able to report the exact nature of the stimulus. In short, autonomic reactivity may have a lower threshold to threat than do those neural systems which mediate consciousness. Study of such reactions, therefore, should hold significant possibilities for adding to our understanding of the process by which discriminatory evaluation of visually sensed objects is accomplished before accurate perception occurs.

THE EXPERIMENT

Because of the ease and precision with which it can be measured, the galvanic skin response was selected in the present study as an index of emotionality [1] in response to affectively-charged verbal symbols. A list of eleven neutral and seven critical, or emotionally toned, words was first devised. The words are listed in Table 1 in their order of presentation to the subjects. Exposure of the words was accomplished by means of a Gerbrand's Mirror Tachistoscope, which allowed controlled variation of the exposure interval from .01 second upward. This was done silently, since exposure duration in this apparatus is controlled by the activation of fluorescent tubes rather than by a shutter arrangement.

[1] The term "emotionality" is employed here in the sense of autonomic response without regard to presence or absence of phenomenological content.

TABLE 1

STIMULUS WORDS USED IN THE EXPERIMENT IN ORDER OF THEIR PRESENTATION TO EACH OBSERVER

Critical, or emotional, words are in italics.

apple	*kotex*
dance	broom
raped	stove
child	*penis*
belly	music
glass	trade
river	*filth*
whore	clear
sleep	*bitch*

Subjects in the experiment were eight male and eight female undergraduates drawn from an elementary psychology class at the University of Alabama. All were naïve as to the purpose of the experiment. The procedure consisted, first, in seating the subject before the viewing mirror of the tachistoscope and strapping electrodes onto both his palms. These were connected in series with a potentiometric circuit described by Lacey and Siegel (**5**) for measuring galvanic skin response. A 32-centimeter scale microammeter accurate to .5 per cent made it possible to read current changes of one microampere with precision. The subject's threshold was first determined for four trial words in order to accustom him to the apparatus and to allow his level of resistance to stabilize. In all cases, thresholds were determined by exposing the stimulus word once at .01 second, once at .02 second, etc. until it was correctly reported by the subject.

Prior to experimentation, the subjects were told that they would be shown words which they might not be

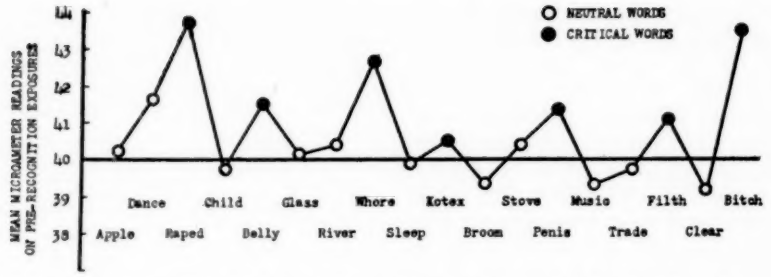

FIG. 1. Group averages of galvanic skin response to neutral and critical words during pre-recognition exposures.

able to recognize at first. They were instructed to report whatever they saw or thought they saw on each exposure, regardless of what it was. One additional injunction was that they fix their hypothesis upon exposure of the stimulus, but withhold stating it verbally until they received a signal from the experimenter. In this manner we were able to expose the stimulus word, note the maximum deflection of the microammeter pointer during the six-second period following exposure, and then record the subject's response. Two experimenters cooperated in the procedure, one operating the tachistoscope, the other recording galvanic skin response and readjusting the current through the subject to 40 microamperes after recognition of each word.[2]

EXPERIMENTAL FINDINGS

Emotionality. Since we were interested primarily in the galvanic skin response of our subjects during the period *preceding correct recognition* of the stimulus words, we have based our analysis upon just those microammeter readings which were recorded on exposure trials up to, but not including, the trial on which recognition finally occurred. Assuming that the GSR may properly be considered an index of "emotionality," we have succeeded in measuring emotional, or autonomic, reactivity to verbal symbols during the period preceding accurate recognition of the stimulus. That emotionality, so defined, is significantly greater during pre-recognition exposures of the critical than of the neutral words is confirmed by statistical analysis of the findings. Testing the null hypothesis that no differences other than those attributable to random fluctuations in the data would exist between mean galvanic skin responses of the observers to the neutral and critical words, we obtained a t value of 5.10 for 15 degrees of freedom. This permits rejection of the null hypothesis at the .01 level of confidence, and indicates a highly significant relationship between GSR and word meaning during the pre-recognition period. The results are presented graphically in Fig. 1, while the experimental findings are summarized in Table 2.

Thresholds. Of equal interest are the data relating to thresholds of the observers for the neutral and critical words. The relationship here is depicted in Fig. 2. Without exception, the mean thresholds of the observers were greater for the critical than for the neutral stimulus words. The sig-

[2] Miss Billie Sue Talantis, a graduate student in psychology at Alabama, assisted in the experimental procedure. The author is also grateful to Rosemary T. McGinnies for her generous assistance in organizing the data.

TABLE 2

SUMMARY OF THE RAW DATA AND STATISTICAL TESTS FOR ALL OBSERVERS WITH RESPECT TO BOTH GALVANIC SKIN RESPONSE AND THRESHOLDS OF RECOGNITION FOR NEUTRAL AND CRITICAL STIMULUS WORDS

Observer	Mean microammeter readings during pre-recognition exposures		Mean thresholds of recognition	
	Neutral words	Critical words	Neutral words	Critical words
1	37.80	40.46	.055	.184
2	40.96	41.53	.044	.094
3	39.31	42.06	.054	.080
4	38.34	40.80	.103	.126
5	41.48	43.76	.040	.064
6	41.41	47.08	.070	.130
7	40.75	39.94	.057	.104
8	39.98	42.85	.063	.076
9	39.44	42.68	.059	.130
10	40.02	42.71	.049	.223
11	39.88	41.55	.046	.077
12	41.27	44.02	.057	.091
13	40.56	41.37	.033	.037
14	40.19	41.42	.034	.054
15	40.85	40.63	.046	.056
16	40.83	41.84	.036	.046
Mean diff. = 1.98 t = 5.10 $P < .01$			Mean diff. = .045 t = 3.96 $P < .01$	

nificance of the individual differences in thresholds between critical and neutral words was tested statistically, the results yielding in this case a t value of 3.96 for 15 degrees of freedom. In short, the observers displayed significantly higher thresholds of recognition for the emotionally-toned words than they did for the neutral words. The threshold measures are also summarized in Table 2.

A breakdown of the data with respect to sex of the observers was also done. The male subjects, on the average, had significantly lower thresholds for both the neutral and critical words. Since the factor of individual differences in visual acuity was not controlled, these results cannot be accepted as more than suggestive of a sex difference in threshold of visual recognition. The mean difference between thresholds for neutral and critical words, however, did not differ significantly in magnitude for the male and female observers. Neither group, in other words, displayed greater evidence of perceptual defense than the other. In addition it was found that no significant sex differences existed with respect to absolute magnitude of galvanic skin response to the critical and neutral words or to differential GSR to the two types of words. Emotionality during the pre-recognition period was of equal degree in both the men and the women.

Content analysis. Since the observ-

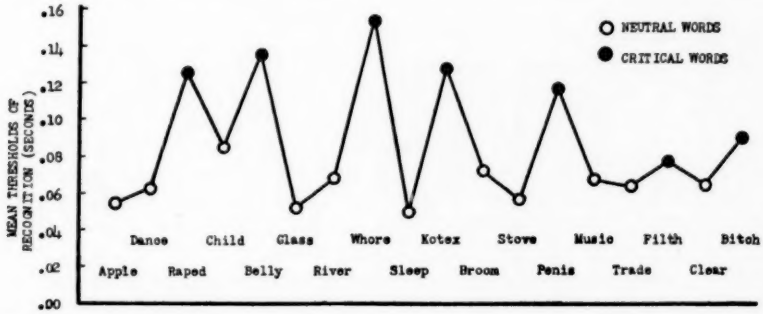

FIG. 2. Mean thresholds of recognition of the observers to the neutral and emotionally-charged words.

ers were instructed to report whatever they saw, they characteristically volunteered a number of pre-recognition "hypotheses" before recognition occurred. These were recorded and later analyzed in terms of four general response categories. In order to simplify and objectify as much as possible the coding of these perceptual "guesses," the content categories were limited to the following:

(1) *Structurally similar.* Hypotheses coded under this heading resembled in structure the stimulus word. For example, the observer may have guessed *trace* for *trade*, or *whose* for *whore*.

(2) *Structurally unlike.* Coded here were hypotheses that were unlike, or dissimilar, in structure to the stimulus word, as, for example, *roared* for *belly*, or *ideal* for *glass*.

(3) *Nonsense.* This category included responses that simply had no dictionary meanings. Such would be the case in guessing *egtry* for *kotex*, or *widge* for *stove*.

(4) *Part.* These were fractional, or incomplete, hypotheses consisting of any disconnected group of letters.

Figure 3 shows the percentage of responses in each of the content categories made to neutral and critical stimulus words for the group of observers as a whole. A Chi-Square test of independence between type of hypothesis and meaning of the stimulus words indicates a relationship significant below the .01 level of confidence. The tabulation of observed and theoretical frequencies with the obtained value of Chi-Square is reproduced in Table 3. Inspection of Fig. 3 in the light of the statistical evidence reveals that the observers made proportionately more *similar* and *part* responses to the neutral words and proportionately more *unlike* and *nonsense* hypotheses to the critical words. An additional breakdown of the *nonsense* category into re-

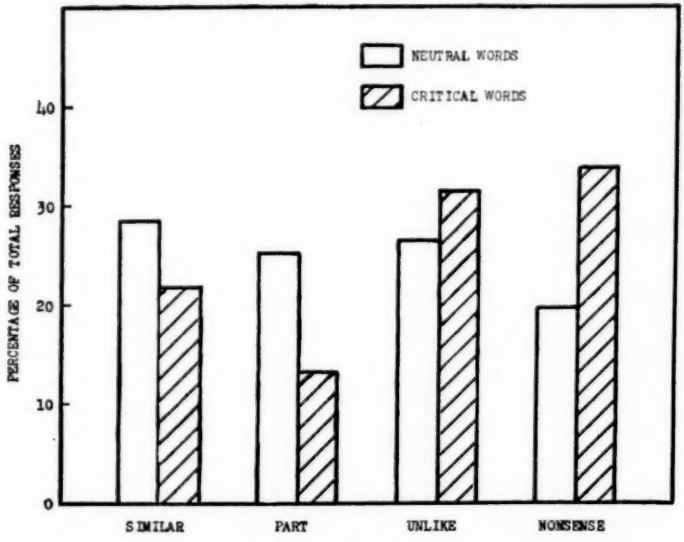

FIG. 3. Percentage frequencies with which hypotheses to neutral and critical stimulus words appeared in the response categories.

sponses which were similar to and those which were unlike the stimulus word failed to reveal any difference in frequency of occurrence to neutral and critical words. For this reason it was concluded that more detailed analysis of the content of responses would be unwarranted and would merely introduce an additional degree of arbitrariness to the scoring of the pre-recognition guesses.

The subjects were queried following the experimental session as to whether they had reported their perceptions of the words promptly and accurately. In all cases, they assured the experimenters that, with the occasional exception of the first charged word, they did not withhold or modify their verbal response because of reluctance to say the word. In measuring the GSR, then, we apparently were recording genuine pre-recognition reactions to the stimulus words.

IMPLICATIONS FOR PERCEPTUAL THEORY

Armed with the findings thus far reported, we can readdress ourselves to the problem of "discrimination without awareness," to employ a term used by J. G. Miller (8). It seems clear that emotional reactivity, as measured by the galvanic skin response, is an accompaniment of perceptual defense. The existence of such a state of affairs has previously been suggested by H. A. Murray who states that ". . . certain features of the object which the subject does not consciously perceive are nevertheless physically affecting his body, and though he may be unable to report upon these internal happenings, they are nevertheless affecting his conscious appraisal of the object" (9, 312). Although Murray was speaking without laboratory evidence, his phrasing of the matter in terms of emotional conditioning is essentially correct.

TABLE 3

CHI-SQUARE TEST OF INDEPENDENCE BETWEEN STIMULUS WORDS AND RESPONSE CATEGORIES

Theoretical frequencies are in parentheses.

	Neutral	Critical	
Similar	89 (76.36)	93 (105.64)	182
Part	79 (57.06)	57 (78.94)	136
Unlike	83 (91.89)	136 (127.11)	219
Nonsense	62 (87.69)	147 (121.31)	209
	313	433	746

$x^2 = 31.26$. $P < .01$.

Early in life, most individuals learn that words like "whore" and "bitch" are socially taboo. Since the use of such words by the child will generally result in chastisement by the parent, a conditioned emotional reaction to these verbal symbols is soon established. This pattern of conditioned emotional response may be considered one of fear or anxiety aroused by symbols having sexual, excretory, or otherwise unpleasant or "immoral" connotations. Despite the fact that these words may be employed frequently at a later age, especially when communicating with members of one's own sex, the early emotional reaction persists, as revealed by the GSR, even when overt signs of anxiety or embarrassment are not observable.

Despite evidence of unconscious emotional arousal, perceptual defense against these anxiety-arousing symbols is still accomplished, as witnessed by the heightened limens of our observers when they were confronted with the charged words. This poses a problem for neurophysiological explanation

which cannot be answered here. However, we might consider two possibilities: Is the galvanic skin response preceding recognition of critical words a result of "feed-back" from the cortical association centers? Or is autonomic response initiated as the visual impulses reach the optic thalamus? In this case, one might conjecture that "rerouting" of afferent activity then takes place in the several visual centers so that cortical integration is effectively modified in the direction of phenomenological distortion. Evidence for this latter hypothesis is found in the greater frequency of *nonsense* and *unlike* hypotheses in response to the charged words. Formulation of these pre-recognition perceptions represents tactics apparently designed to delay accurate recognition of the stimulus word. The relatively higher frequency of *part* responses to the neutral words, on the other hand, may indicate effort toward recognition. That is, hypotheses based upon the neutral words are not as frequently distorted into *nonsense* or *structurally dissimilar* percepts, but are based upon whatever fractional discriminations the observer can make. Such an explanation, of course, is *ad hoc* and is presented as such.

It has been suggested to the author by Dr. Jerome Bruner that an alternative explanation of the findings might be summarized as follows: The "critical" words appear less frequently in print, and the increase in thresholds for these words is a function of their unfamiliarity. Greater "effort" is required to recognize them, and this, in turn, causes a heightened GSR to the critical words. Such an explanation, however, seems untenable on several grounds. First, the critical words are quite common in conversational usage despite their infrequent appearance in print. Second, there is no reason why unfamiliarity with these words should generate a preponderance of *nonsense* and *structurally unlike* hypotheses. Third, if GSR is merely an accompaniment of the increased effort expended in recognizing words which show higher thresholds, one should expect a correlation between mean GSR's and mean thresholds for both the neutral and critical words. Pearson *r*'s were calculated in each case. Correlations of $-.002$ and $+.077$ were obtained between mean GSR's and mean thresholds for the neutral and critical words respectively. Clearly, no significant relationship exists within the two groups of words between GSR and threshold. The results, therefore, may be viewed as reflecting genuine emotional response rather than mere autonomic reactivity accompanying effort at recognition.

Perceptual defense apparently is based upon conditioned avoidance of unpleasant or dangerous stimulus objects. That the individual actually discriminates the stimulus before he fully perceives it is evident in his increased emotionality before recognition. Inimical stimuli, then, may serve as cues which are appropriately evaluated by the central nervous system even though integration of the afferent impulses is such as to delay recognition, either through distortion or an increase in threshold or both. Almost without exception, the galvanic skin response of the observers was greatest following the final exposure of the critical words; that is the one during which recognition occurred. Clearly, the process of perceptual defense is designed to delay the greater anxiety that accompanies actual recognition of the stimulus. As suggested previously, some integrational processes may occur at the thalamic level which are effective in delaying or modifying cortical integration of visual patterns, while at the same time causing autonomic reaction to emotionally meaningful stimuli. The conditioned response, anxiety, is not entirely circumvented, even though perceptual avoidance is in some measure achieved.

SUMMARY

Recognition thresholds and galvanic skin responses during the pre-recognition period were measured for sixteen observers presented tachistoscopically with eleven neutral and seven emotionally-toned words, randomly ordered. The observers reacted with GSR's of significantly greater magnitude during the pre-recognition presentation of the critical words than they did before recognizing the neutral words. In addition, the observers displayed significantly higher thresholds of recognition for the critical than for the neutral words. Hypotheses made before recognition of the charged words were of such a nature as to indicate resistance to recognizing these words. The findings are interpreted as representing conditioned avoidance of verbal symbols having unpleasant meanings to the observer. The stimulus word serves as a cue to deeply imbedded anxiety which is revealed in autonomic reactivity as measured by the GSR. Avoidance of further anxiety is contemporaneously aroused in the form of perceptual defense against recognition of the stimulus object.

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[MS. received April 8, 1949]

HULL'S DERIVATION OF STIMULUS ASYNCHRONISM: A CORRECTION

BY J. M. HARRISON

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INTRODUCTION

One of the advantages of a deductive system as presented in Hull's *Principles of Behavior* (5) is that if derivations from the fundamental postulates fail to agree with empirical observation, then the postulates can be modified or increased in number. The author intends to show an inadequacy in the postulates used in the *Principles of Behavior* and then to add a postulate which may serve to remedy the defect. This added postulate is applied only to a single behavioral field in this paper, namely the derivation of stimulus asynchronism; application of this postulate to other behavioral fields will be developed and clarified in a subsequent paper. Stimulus asynchronism is used in the literature to denote the empirical finding that for optimum rate of learning to take place in a typical conditioned response experiment the conditioned stimulus must precede the unconditioned stimulus by a time interval which is probably somewhere between 300 and 1800 m/s. The existence of stimulus asynchronism is well established and a few of the experimental findings will be briefly reviewed to define the limits of this asynchronism. The manner in which Hull derives stimulus asynchronism in his *Principles of Behavior* will be considered in detail and it will be shown that the actual derivation is apparently faulty. A stimulus asynchronism curve will be derived from the combination of the additional postulate and those of Hull already existing.

THE EXPERIMENTALLY DETERMINED MAGNITUDE OF STIMULUS ASYNCHRONISM

In the typical conditioning experiment there are three main factors of interest, the unconditioned stimulus (S_u), the conditioned stimulus (S_c) and the unconditioned response (R). After S_c and S_u are applied repeatedly and at approximately the same instant it is found that if S_c is applied alone, the unconditioned response or some portion of it is produced. At first sight it might appear that, for the most efficient formation of the conditioned response, the conditioned and the unconditioned stimuli should be applied simultaneously rather than with a temporal separation between them. But detailed experimental work has shown that this is not so. One of the earliest investigations in this field made by Wolfle (12) showed that the most favorable separation of S_c and S_u was something of the order of 500 m/s. In a later experiment (13) the same author found that the optimum separation of the two stimuli was about 200 m/s. Bernstein (1) measured the efficiency of the conditioning of the eyelid response to a sound of short duration and found that the most efficient learning occurred with intervals of about 300 m/s with little decline in efficiency for periods up to 1800 m/s. Kappauf and Schlosberg (6) made use of a "delayed" conditioning experiment and found that the optimum interval was somewhere between 667 and 1000 m/s depending upon the criterion of conditioning chosen. Reynolds (8), who

TABLE I

Investigator	S_c-S_u separation	Type of experiment
Wolfle (1930)	500 m/s	Trace avoidance
Wolfle (1932)	300 m/s	Trace avoidance
Bernstein (1934)	300–1480 m/s	Trace
Kappauf and Schlosberg (1937)	667–1000 m/s	Delayed
Reynolds (1945)	450 m/s	Trace
Kimble (1947)	Around 400 m/s	Delayed

made use of the blink response, found that for optimum conditioning the delay of the unconditioned stimulus was around 450 m/s. Finally, Kimble (**7**), in a carefully planned experiment, has shown that the most efficient learning takes place when the separation is around 400 m/s. From these studies there appears to be little doubt of the empirical fact that there is an optimum delay between S_c and S_u for the most efficient formation of conditioned responses. These findings are summarized in Table I.

If we omit the lack of agreement as to the precise value of the optimum separation we may generalize the findings in Table I as follows:

In a conditioned response experiment the formation of the conditioned response is most efficient when the conditioned stimulus precedes the unconditioned stimulus by a short time interval. If the interval by which S_c precedes S_u is greater or smaller than the optimum period, the conditioning will be less efficient.

This statement is referred to as stimulus asynchronism and it is the type of empirical finding which Hull is interested in deriving from a set of more fundamental postulates.

HULL'S DERIVATION OF STIMULUS ASYNCHRONISM

In the type of scientific explanation adopted by Hull an attempt is made to formulate a series of primary postulates (from the general fund of knowledge) and to derive from these, as theorems, a large number of predictions of empirical events which are capable of experimental or observational verification. If these deductions are confirmed by the results of various experiments, the primary postulates may also be confirmed. If the experimental findings are contrary to the predictions the initial postulates must be revised. With such a system one can avoid the use of a large number of *ad hoc* explanations and can also arrive at a logical and systematic set of relationships between apparently isolated data purely in terms of the postulates. The beginning of such a system of postulates and deductions has been made in Hull's *Principles of Behavior* (**5**), and he has derived a large variety of detailed behavioral phenomena from a relatively small number of postulates.

The existence of S_c–S_u asynchronism is an empirical finding which Hull derives from elementary postulates and it is with this derivation that we are concerned.

Let us consider the relevant postulates and assumptions which Hull uses. The first assumption concerns the relationship between the "afferent neural impulse" and the application of a stimulus to a receptor organ (Hull's postulate I). According to this postulate the application of a stimulus, *S*, to a receptor produces, after a short interval, an afferent impulse, *s*. The intensity of the "afferent neural impulse" rapidly rises to a maximum value, following which it decays to zero or a low value depending upon the receptor. After the termination of the action of the stimulus, *S*, on the receptor, the afferent impulse, *s*, continues its activity in the central nervous tissue for some seconds, gradually diminishing to zero as a simple decay function of its

value at the time the stimulus, S, ceases to act. The time interval between the application of the stimulus, S, and the afferent impulse, s (considered here as an event of the receptor areas of the brain), will be referred to as receptor latency, t. Secondly, Hull makes use of the laws of primary and secondary reinforcement (actually the first part of Postulate IV, **5**, p. 178). According to these two laws whenever an effector activity ($r \rightarrow R$) and a receptor activity ($S \rightarrow s$) occur in close temporal contiguity and when this occurrence is followed by a primary or secondary reinforcing state of affairs, then there is an increment of learning between s and r such that on later presentations of S, R is more likely to occur than it was prior to the reinforced trial. Learning is thus fundamentally the joining of an independent afferent event and an independent efferent event. Finally, we have Hull's statement of the Kappauf-Schlosberg hypothesis which assumes that the magnitude of the increment of learning ($\Delta_s H_r$) taking place between s and r, other things being equal, is an increasing function of the intensity of discharge of the afferent system. The greater the magnitude of s the greater the increment of learning when all other factors remain constant. We now have all the material from which Hull proceeds to derive Corollary I and Corollary IV (**5**, p. 169). These are quoted in full:

Corollary I. "It follows from the above hypothesis [Kappauf-Schlosberg] and empirical item 3 [receptor latency] that in a reinforcement situation there is a temporal relationship of the conditioned to the unconditioned stimulus such that as the onset of the unconditioned stimulus is progressively delayed, the rate of learning will increase."

Corollary IV. "It follows from the hypothesis [Kappauf-Schlosberg] and empirical item 6 [decay of afferent impulse frequency after the period of the receptor latency] that in a reinforcement situation as the time of the onset of the unconditioned stimulus is further retarded beyond the point of optimal timing, the rate of learning will decline, the course of the decline following a negative growth function of the amount of delay, with asymptote appreciably above zero in the case of certain receptors."

In so far as these two deductions are valid conclusions from the initial data they are derivations, taken together, of the empirical event of stimulus asynchronism found in the investigations cited at the beginning of this paper and the initial assumptions from which the corollaries were deduced are therefore supported by these deductions. But this state of affairs only exists if the deductions are logically valid and this, unfortunately, does not appear to be the case. To demonstrate this all the steps involved in the deduction of the S_c–S_u separation for optimum learning are given below. The assumption is made that s and r occur simultaneously at their maximum intensities in order to undergo a maximum increment of learning.

Let:

S_u be a stimulus to which is attached (by previous learning or inborn connection) a response R.

S_c be another independent stimulus to which no predominant response is attached.

s_u be the afferent counterpart of S_u.

s_c be the afferent counterpart of S_c.

r be the efferent counterpart of R.

t_u be the receptor latency of s_u on S_u.

t_c be the receptor latency of s_c and S_c.

t_u approximately equal t_c.

And let the interval between r and s_u (on which r is dependent) be sufficiently small to be ignored.

Now, if S_c is applied to an organism at any time, t_o, the afferent event, s_c, will occur at a time:

$$\text{(Time of occurrence of } s_c) = t_o + t_c. \quad \text{(i)}$$

Similarly, if S_u is applied to an organism at any time, t_o', the efferent event, r, will occur at a time:

$$\text{(Time of occurrence of } r) = t_o' + t_u. \quad \text{(ii)}$$

Since s_c and r are simultaneous for a maximum increment of learning, we may write:

$$t_o' + t_u = t_o + t_c. \quad \text{(iii)}$$

Therefore:

$$t_o' = t_o. \quad \text{(iv)}$$

Equation (iv) defines the condition in which S_c and S_u are simultaneous. It thus appears that Hull's system leads to the *derivation* that S_c and S_u must be applied simultaneously for optimum learning to take place. Hull, however, points out that the receptor latency is a function of the intensity of the impinging stimulus, S. Hence we can readily imagine the condition in which either $t_u < t_c$ or in which $t_u > t_c$. In the first case it will be deduced that S_c will precede S_u by the difference between t_u and t_c, and in the latter case S_u will precede S_c by the difference between t_u and t_c.

It can readily be seen that if S_u is progressively delayed, r will occur at a time later than the occurrence of the maximum value of s_c and that learning will thus decrease. In other words, for the condition in which $t_c = t_u$ (approximately), Corollary I is not a deduction from the initial set of assumptions. The actual deductions possible from the basic assumptions are apparently indeterminate (with regard to the order of occurrence of S_c and S_u) unless the difference of the afferent latencies for the conditioned and the unconditioned stimuli is known, and then those two deductions made for the conditions $t_u = t_c$ and $t_u > t_c$ would be logically valid but would not agree with empirical observation.

Kimble (7) has pointed out that some of Hull's neurological data require modification to be brought more into line with modern findings. It does not appear, however, even if we modify the neurological data and make some additional assumptions, that stimulus asynchronism can be derived from the general Hullian approach. Kimble, who quotes Riggs, Bartley and others, points out that the interval between the application of a stimulus to a receptor and the appearance of the first afferent impulse is of the order of 100 m/s rather than several hundred as assumed by Hull. If the afferent impulses generated at the receptor are to take part in a learned connection, they must reach some portion of the brain. Since neural conduction has a finite velocity, these afferent impulses will reach the brain later than they are generated at the receptor organs. If we assume a conduction velocity of 100 metres/sec. (4, p. 188) and a mean path length of 50 cms., it will take the afferent impulses 5 m/s to reach the posterior spinal nerve root concerned. The number of synaptic connections between the spinal nerve root and the receptor areas of the brain is not known, so we can only guess that the time of transmission along this path will be of the order of 10 m/s. If we sum these three intervals, we can estimate that the time of occurrence of maximum activity of the receptor areas of the brain will be some 115 m/s after the application of the stimulus. Before any movement can take place after the application of a stimulus, the activity of the receptor areas of the brain must reach the motor areas of the brain, and here again we

can only guess at a figure of 10 m/s as the time of the transmission between the two central areas. The temporal lag between the occurrence of the activity of the motor areas and the operation of the effector is composed of the time of transmission through the efferent nervous system and the latency of the effector organs themselves. The time of transmission through the efferent nervous system we can assume to be approximately the same as for the afferent system, namely about 15 m/s, and the latency of the effector organs would appear to have an average of about 20 m/s (**4**, p. 130, Fig. 55).

Table II summarizes the above time estimates when any stimulus, S, is applied to an organism at a time, t.

TABLE II

Event	Time of occurrence: m/s
Application of S	t
Maximum discharge of receptor	$t+100$
Arrival of afferent impulse at central areas	$t+115$
Maximum activity of central efferent areas	$t+125$
Maximum peripheral efferent nerve activity	$t+140$
Time of operation of effector	$t+165$

We can now proceed to derive, from the data of Table II and Hull's assumptions, the S_e–S_u separation for optimum learning. We will make use of the same notation as before and will also assume that the two receptor latencies, t_e and t_u, are approximately equal. We will again use as the reference point the fact s_e (central event due to S_e) and r (central event due to S_u) must be at their maximum values simultaneously for the maximum increment of learning to occur.

S_e gives rise to s_e 115 m/s after the application of S_e (Table II).

S_u gives rise to s_u 115 m/s after the application of S_u (Table II).

s_u gives rise to r 10 m/s after the occurrence of s_u (Table II).

S_e will occur 115 m/s before the simultaneous occurrence of s_e and r, and S_u will occur $115 + 10$ m/s $= 125$ m/s before the simultaneous occurrence of s_e and r. Thus, we derive that S_u will have occurred $125 - 115$ m/s $= 10$ m/s before S_e. We can thus see that, even if we take into account the various delay times in the central nervous system and elsewhere, we find S_u must precede S_e, a deduction which still does not fit the empirical findings. The only way we can arrive at a derivation in which S_e antedates S_u is by making the assumption that the latency between S_u and s_u is very much smaller than that between S_e and s_e. This assumption may be justified with regard to certain kinds of stimuli and sense organs, but Hull's system refers not only to certain restricted ones but to all receptor–effector connections. Such an assumption is too restricting for us to make. Thus Hull's logarithmic relationship between the magnitudes of $\Delta_s H_r$ and the stimulus asynchronism becomes an empirical equation fitted to the data rather than a rationally derived equation. This lack of agreement between deduction and empirical fact implies that a fault exists in the postulate system and renders corollaries I to V (**5**, pp. 168–169) correct statements of empirical fact but incorrect deductions from the postulates and the Kappauf-Schlosberg hypothesis.

A POSSIBLE CORRECTION TO HULL'S SYSTEM

Implicit in most "behaviorist" theoretical systems is the assumption that the nervous system may be considered (at least at the molar level) as a mechanism in which efferent impulses are more or less directly dependent (with "amplification") upon afferent impulses. While it is recognized that the trans-

mission of neural impulses occupies a time interval, the system is supposed to operate in a smooth and continuous manner irrespective of the temporal separation of the groups of afferent impulses (except for extremely short temporal separations). The time interval between the application of a stimulus and the occurrence of a response is referred to as the reaction time, R/T, of the system under the stated conditions. Thus, on this assumption of continuous functioning, if two visual stimuli occur t seconds apart then the two responses associated with these stimuli will also occur t seconds apart and (R/T) and $(R/T + t)$ seconds later, respectively, for all values of t above a very small minimum. There is, however, experimental evidence which indicates that this simple relationship does not hold for values of t between 0 and some 500 m/s and that the nervous system ceases to function in a smooth and continuous manner for time intervals of this order.

The simple reaction time experiment (*i.e.*, one involving no "choice") involves, in effect, two stimuli and a single response. The first stimulus is the "warning" and the second is that to which, it is expected, the subject will respond as rapidly as possible. The time between the stimuli will be referred to as the warning period. The relationship between the length of the warning period and the mean R/T in the simple reaction time experiment has been a matter of some interest to psychologists and a representative selection of experiments will be found in Woodworth (11, Chap. 14). We may generalize these findings by saying that for warning periods of 1 to 24 seconds the R/T shows an increase with increase in the warning period. This is compatible with the assumption of a "continuously" acting central nervous system, in that the separation of the two stimuli has only a relatively small (4 to 20 m/s, depending upon the experimental conditions) effect upon the R/T. In experiments in which warnings of less than 1 second were used the variations in R/T were considerable. Telford (9), who made use of warning periods of between 0.5 and 4 seconds, found that the R/T at the minimum warning period was about 100 m/s longer than for periods of 1 or 2 seconds and about 60 m/s longer than the R/T at the maximum warning period. Craik (2, 3) has also reported that if two stimuli are presented within 50 to 500 m/s of each other, the R/T to the second stimulus is delayed and as the warning period approaches 50 m/s the second stimulus tends to be completely missed (not responded to). Finally, we may consider the work of Vince (10) in which two visual stimuli of the same magnitude were presented to the subject at various separation periods between 50 m/s and 1600 m/s. The R/T to the first stimulus was about the same as that to the second when temporal separations of stimuli were above 500 m/s, but as the separation fell below this figure the R/T to the second stimulus was progressively lengthened to a maximum value of about 500 m/s which occurred at the 50 m/s warning period. At this latter value of warning period 21.21 per cent of the second stimuli were missed. If we reduced the separation to less than 50 m/s we might expect that the percentage of missed stimuli would approach 100 per cent. Let us generalize this material as follows:

(*a*) If any two stimuli are presented within 0 to 50 m/s of each other, the second stimulus tends to be missed in an increasing proportion of the number of presentations as the separation approaches 0 m/s.

(*b*) If any two stimuli are presented within 50 to 500 m/s of each other the reaction time to the second stimulus is

progressively lengthened to the limit of 500 m/s as the stimulus separation approaches 50 m/s.

It is advisable to point out that no reference to the perceptual events associated with two stimuli having a small temporal separation is made or intended in this paper. There is certainly evidence (11, Chap. 21) which indicates that two aural stimuli having a short temporal separation may be perceived as a dual event, but this perception is considered irrelevant to the organism's ability to react separately to these two stimuli, *i.e.*, with a temporal spacing of the reactions equal to the actual or perceived spacing of the two stimuli.

The generalizations of the findings for short warning periods suggest that perhaps the nervous system, after the application of a stimulus, enters into a relative refractory phase, during which it is relatively unresponsive to further stimulation. Such a suggestion is not compatible with a "continuously" operating system and we might have to deal with a mechanism that has some of the characteristics of an "intermittent" (non-continuous) transmission system. While we are not in a position to derive such "intermittent" functioning, we may assume this "intermittent" characteristic as a fundamental postulate and introduce it into Hull's system under the name of *stimulus inhibition* (I_s), to keep within Hull's notation. If such a concept is to be of any value it must be anchored to the observables in a manner which allows its influence to be calculated. We may also consider stimulus inhibition to be equivalent to a rise of threshold of the organism's receptor system, the larger the stimulus inhibition the higher the threshold. As a first approximation let us assume that the *maximum* value of I_s is a simple linear function of the intensity of the applied stimulus, S.

Thus:

$$I_s = CS \qquad \text{(v)}$$

where C is a constant.

From generalization (*a*) above it is apparent that I_s reaches its maximum value within about 50 m/s of the application of the stimulus responsible for its generation. After a total period of some 500 m/s, see generalization (*b*), it has apparently decayed to its minimum value. Let us assume a simple exponential decay. Since we have considered I_s as equivalent to an increase in threshold of the receptor system, we may make use of the absolute sensory threshold as a measure of the minimum value of I_s. If we term this "threshold inhibition," I_t, and assume exponential decay, we may write:

$$I_s' = I_t + I_s e^{-h(t-t_s)} \qquad \text{(vi)}$$

where I_s' is the instantaneous value of I_s,
h is a constant,
t_s is the receptor latency,
t is the time since the application of S.

Expression (vi) is assumed to hold for all positive values of $(t - t_s)$ only.

It now remains to obtain a measure of the maximum value of I_s'. An increase in effective threshold is the same (as a first approximation) as an effective decrease in the magnitude of any stimulus applied to the system; so the effect of any existing I_s' will be to reduce the magnitude of any stimulus applied at the time. We saw from Vince's work that, when two stimuli of equal magnitude are applied within 50 m/s of each other, the second stimulus is missed some 21 per cent of the time. If we rephrase this result in terms of changes in threshold, we may say that the threshold of the system has been raised to such an extent that the second stimulus is somewhere near this new

threshold value. (The second stimulus would be at threshold value if it were missed 50 per cent of the time.) So it appears that we can define the maximum value of the effective threshold (that is the maximum value of I_s) in terms of the magnitude of the stimulus S, which has given rise to the stimulus inhibition. We may state this formally as follows:

Any stimulus, S, applied to an organism gives rise to a central event, s, which in turn gives rise to a factor of stimulus inhibition, I_s, the maximum value of which is such that the effective threshold of the system considered is about equal to the magnitude of the applied stimulus, S.

From this it immediately follows that if a second stimulus, S_u (of equal magnitude to a first stimulus, S_c), is applied to an organism after the application of a first stimulus, S_c, the effective magnitude of the second stimulus, S_u, decreases to zero as the time of separation of S_c and S_u decreases to zero. If we substitute the central event, s_u, for the stimulus, S_u, we may write:

$$s_u' = s_u(1 - e^{-k(t-t_u)}) \qquad \text{(vii)}$$

where s_u' is the instantaneous value of s_u,
s_u is the maximum value in the absence of I_s,
k is a constant,
t is the time elapsed since the application of S_c,
t_u is the receptor latency of S_u.

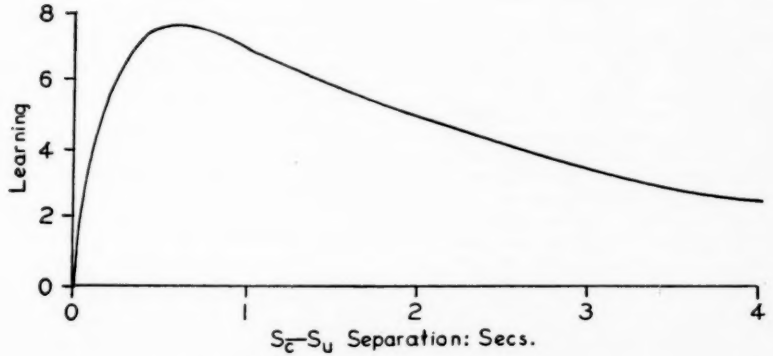
This expression holds for all positive values of $(t - t_u)$.

If we admit stimulus inhibition as a primary molar postulate to Hull's system, we may combine it with the existing postulates. We have seen from expression (vii) that I_s reduces the effective magnitude of the unconditioned afferent impulse, s_u. If we derive stimulus asynchronism by Hull's method but take into account the effect of stimulus inhibition (generated by s_c) we have an equation which includes the multiplicative result of the instantaneous magnitude of s_c and s_u. We may write this:

$$\text{Effective learning} = s_u(1 - e^{-k(t-t_u)})(s_c e^{-j(t-t_u)}) \qquad \text{(viii)}$$

where j is a constant and other symbols are as used above.

This function has been plotted in the figure for arbitrary and equal magnitude of s_c and s_u, equal receptor latencies and for constant k equal to 2 and the constant j equal to .15. Inspection of the figure suggests that, under the conditions stated, optimum learning will be distributed over a fairly large



time interval, whereas inspection of Hull's (**5**, p. 170, Fig. 39) derived curve of stimulus asynchronism suggests a sharply defined point of optimum learning. Inspection of the learning curves obtained in the investigations quoted at the beginning of this paper does not fully confirm the relationship plotted in the figure; but there is, in these same investigations, evidence of the existence of a fairly large time interval during which there is little change in learning efficiency.

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THE EXPRESSIVE COMPONENT OF BEHAVIOR [1]

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The distinction between the expressive (non-instrumental) and the coping (instrumental, adaptive, functional, purposive) components of behavior is by now well established (at least among dynamic psychologists) but has not yet been properly exploited as a basis for value-psychology.[2]

Because contemporary psychology is overly pragmatic, it abdicates from certain areas that should be of great concern to it. In its preoccupation with practical results, with technology and means (29), it has notoriously little to say, for example, about beauty, art, fun, play, wonder, awe, joy, love, happiness and other "useless" reactions. It is therefore of little or no service, as compared, say, with psychoanalysis, to the artist, the musician, the poet, the novelist, to the humanist, the connoisseur, the axiologist, the theologian or to other end- or enjoyment-oriented individuals. This is the equivalent of an accusation against psychology that it offers little to the modern man whose most desperate need is a naturalistic or humanistic value-system.

By exploring and applying the differentiation between expression and coping—which is simultaneously a differentiation between "useless" and "useful" behavior—we may help to extend the jurisdiction of psychology in these desirable directions. This paper also is conceived as a necessary preliminary to the important task of challenging and calling into question the generally accepted belief that all behavior is motivated. This will be attempted in another paper.

For the time being, it is desirable to consider these problems as distinct and different from each other. Our present problem deals with the *effects* or results of behavior; the discussion of unmotivated behavior comes under the head of the *determinants* of behavior. The present paper discusses the differences between expression and coping [3] and then applies them to some problems of psychopathology.

1. Coping is by definition purposive and motivated; expression is most often unmotivated.

2. Coping is more determined by environmental and cultural variables; expression is largely determined by the state of the organism. A corollary is the much higher correlation of expression with deep-lying character structure.

3. Coping is most often learned; expression most often unlearned.

4. Coping is more easily controlled

[1] This paper was first read before the 1943 meeting of the Society for Research in Psychoanalysis and Psychosomatics in New York City. I wish to thank Mrs. Fern McGrath for her help in preparing this paper.

[2] This is one of several theoretical constructions pointing toward a dynamic-holistic psychology of positive values. By positive I mean derived from the empirical study of the healthy organism rather than from psychopathology alone.

[3] As always, in presenting a distinction, we must be careful to avoid sharp, either-or dichotomizing. Most acts of behavior have both an expressive and a coping component, *e.g.*, walking has simultaneously a purpose and a style. And yet we do not wish to exclude, as do Allport and Vernon (3), the theoretical possibility of practically pure expressive acts, *e.g.*, sauntering instead of walking, a child's laughing in glee, private, noncommunicative artistic activity, pure self-actualization, etc. On the other hand, we gain nothing by blurring this fruitful and even necessary distinction, as do Kluckhohn and Murray (18) by attributing all expression to a putative "need for activity."

(repressed, suppressed, inhibited, acculturated); expression is often uncontrolled and even uncontrollable.

5. Coping is usually designed to cause changes in the environment and often does; expression is not "designed" to do anything. If it causes environmental changes, it does so unwittingly.

6. Coping is characteristically means-behavior, the end being need-gratification or threat-reduction. Expression is often an end-in-itself.

7. Typically, the coping component is conscious (although it *may* become unconscious); expression is more often not conscious.

I. COPING AND EXPRESSION

Coping behavior always has among its determinants drives, needs, goals, purposes, functions or aims. It comes into existence to get something done, *e.g.*, walking to some destination, shopping for food, going to mail a letter, building a set of bookshelves, or doing the work for which we get paid. The term "coping" itself implies the attempt to solve a problem or at least to deal with it. It therefore implies a reference to something beyond itself; it is not self-contained. This reference may be either to immediate or to basic needs, to means as well as ends, to frustration-induced behavior as well as to goal-seeking behavior (26).

Expressive behavior of the type so far discussed by psychologists (2, 6, 18, 19, 35, 38, 39, 40) is generally unmotivated, although of course it is "determined." (That is, though expressive behavior has many determinants, need-gratification need not be one of them.) It simply mirrors, reflects, signifies or expresses some state of the organism. Indeed it most often *is* part of that state, *e.g.*, the stupidity of the moron, the smile and the springy walk of the healthy person, the benevolent mien of the kind and affectionate, the beauty of the beautiful woman, the slumping posture, lowered tonus, and hopeless expression of the depressed person, the style of handwriting, walking, smiling, dancing, etc. These are non-purposive. They have no aim and no goal. They were not elaborated for the sake of need gratification.[4]

While all this is true as far as it goes, a special problem is raised by what at first glance seems a paradox, namely, the concept of motivated self-expression. The more sophisticated person can *try* to be honest, graceful, kind or even artless. People who have been through psychoanalysis as well as people at the highest motivational levels know well how this is. Indeed it is their most basic single problem. Self-acceptance and spontaneity are among the easiest achievements, *e.g.*, in healthy children, and the most difficult, *e.g.*, in self-questioning, self-improving adults, especially those who have been or still are neurotic. Indeed for some it is an impossible achievement, *e.g.*, in certain types of neurosis the individual is an actor who has no self at all in the ordinary sense, but only a repertory of roles from which to choose.

We may take two examples, one a simple one and the other a very complex one, to demonstrate the (apparent) contradictions which are involved in the concept of motivated, purposeful spontaneity. The most desirable way to dance, at least for the amateur, is to be spontaneous, fluid, automatically responsive to the rhythm of the music, and the unconscious wishes of the partner. A good dancer can "let himself go," becoming a passive instrument

[4] This statement is independent of any particular phrasing of motivation theory. For instance, it applies as well to simple hedonism; thus we may rephrase our statement to say: Coping behavior is responsive to praise or blame, reward or punishment; expressive behavior is ordinarily not, at least so long as it remains expressive.

fashioned by the music and played upon by it. He need have no wish, no criticism, no direction, no self even. In a very real and useful sense of the word, he may become "passive," even as he whirls, glides, and dances to the point of exhaustion. Such passive spontaneity or willing abandon can yield some of life's greatest pleasures, as in allowing the surf to tumble one about, or allowing oneself to be cared for and nursed, as in being made love to, or as in the mother who passively allows her baby to suckle, to bite, and to crawl over her.

But few people can dance as well as this. Most will *try*, will be directed, self-controlled and purposeful, will listen carefully to the rhythm of the music, and by a conscious act of choice, fall in with it. In a word, they will be poor dancers from the point of view of the onlooker and from the subjective point of view as well, for they will never enjoy dancing as a profound experience of self-forgetfulness, and voluntary renunciation of control.

Most good dancers become good without training. And yet education can be a help here too. But it must be a different kind, an education in spontaneity and eager abandon, in being natural, non-voluntary, non-critical and passive. One must "learn" to drop inhibitions, self-consciousness, acculturation, and dignity.

More difficult problems are raised by an examination of the nature of the problem of self-actualization. Of people who are at this level of motivational development, it may be said that their actions and creations are in a very high degree spontaneous and uncriticized and therefore expressive. Furthermore, their motivations change in quality so much and are so different from the ordinary needs for safety or love or respect, that they ought not even be called by the same name.

If the wish for love be called a need, then the pressure to self-actualize ought to be called by some name other than need, since it has so many different characteristics. Since this is not the place for detail, we shall restrict ourselves to pointing out the one main difference most pertinent to our present task, namely, that love and respect, etc. may be considered as external qualities which the organism lacks and therefore needs. Self-actualization is not a lack or deficiency in this sense. It is not something extrinsic to the organism which the organism needs for health, as for example, a tree needs water. Self-actualization is intrinsic growth of what is already *in* the organism, or more accurately of what *is* the organism itself. Just as our tree needs food, sun, water from the environment, so does the person need safety, love and status from the social environment. But as in the first case, so also in the second, this is just where real development, *i.e.*, of individuality, begins. *All* trees need sunlight and *all* human beings need love, and yet, once satiated with these elementary necessities, each tree and each human being proceeds to develop in his own style, uniquely, using these universal necessities to his own private purposes. In a word, development then proceeds from within rather than from without and paradoxically the highest motive is to be unmotivated, *i.e.*, to behave purely expressively. Or, to say it in another way, self-actualization is growth motivation rather than deficiency motivation.

One can *try* to go in a direction of self-actualization by solving the less, prerequisite motivational problems. Thereby one consciously and purposefully seeks spontaneity.

Thus at the highest levels of human development, the distinction between coping and expression, like so many other psychological dichotomies, is resolved and becomes useless.

Inner and outer determinants. Coping behavior is characteristically more determined by relatively external determinants than is expressive behavior. It is most often a functional response to an emergency, a problem or a need whose solution or gratification comes from the physical and/or cultural world. Ultimately, as we have seen, it is an attempt to make up internal deficiencies by external satisfiers.

Expressive behavior contrasts with coping behavior in its more exclusively characterological determination (see below). We may say that coping behavior is *essentially* an interaction of the character with the world, adjusting each to the other with mutual effect; expression is *essentially* an epiphenomenon of the nature of the character structure. In the former, therefore, may be detected the working of both the laws of the physical world and of the inner character; in the latter one detects primarily psychological or characterological laws. An illustration could be the contrast between representational and non-representational art.

Several corollaries follow. (1) It is certain that if one wishes to know about the character structure, the best behavior to study is expressive rather than coping behavior. This is confirmed by the now extensive experience with projective (expressive) tests. (2) With reference to the perennial debate about what is psychology and what is the best approach to its study, it is clear that adjustmental, purposive, motivated, coping behavior is not the only kind of behavior there is, nor is it the most fruitful to study. (3) Our distinction may have some bearing on the question of the continuity or discontinuity of psychology with the other sciences. In principle the study of the natural world should help us to understand coping behavior but probably *not* expression. The latter seems to be more purely "psychological," probably having its own rules and laws and therefore best studied directly rather than through the physical and natural sciences.

Relation to learning. Ideal coping behavior is *characteristically* learned, while ideal expressive behavior is *characteristically* unlearned. We do not have to learn how to feel helpless or look healthy or be stupid or show anger, while we *do* ordinarily have to learn how to build book shelves, ride a bicycle or dress ourselves. This contrast may be clearly seen in the determinants of reaction to achievement tests on the one hand, and to the Rorschach test on the other.

The possibility of control. Differential determination by inner and outer determinants shows itself also in a varying susceptibility to conscious or unconscious control (inhibition, repression, suppression). Spontaneous expression is very difficult to manage, to change, to conceal, to control or to influence in any way. Indeed, control and expression are by definition antithetical. This is true even for the motivated self-expression spoken of above, for this is the end product of a series of efforts to learn how *not* to control.

Control of style of handwriting, dancing, singing, speaking, emotional reacting may at best be kept up for only a short time. Supervision or criticism of one's reactions cannot be continuous. Sooner or later because of fatigue, distraction, redirection of attention, etc., control slips, and deeper, less conscious, more automatic, more characterological determinants take over (11). Expression is not, in the full sense, voluntary behavior.

Effects upon environment. Coping behavior characteristically originates as an attempt to change the world, and characteristically does so with more or less success. Expressive behavior, on

the other hand, often has no effect upon the environment. And where it *does* have such effect, it is not premeditated, willed, or purposed; it is unwitting.

As an example we may take a man in conversation. Conversation has purpose, *e.g.*, he is a salesman trying to get an order, and the conversation is consciously and avowedly brought into being for this reason. But his style of speaking may be unconsciously hostile or snobbish or supercilious and may cause him to lose the order. Thus the expressive aspects of his behavior may have environmental effects but it is to be noted that the speaker did not *want* these effects, that he did not *try* to be supercilious or hostile, and he was not even aware that he gave this impression. The environmental effects of expression, when there are any at all, are unmotivated and epiphenomenal.

Means and ends. Coping behavior is always instrumental, always a means to a motivational end. Contrariwise, any means-end behavior (with the one exception discussed above, of voluntarily giving up coping) must be coping behavior.

On the other hand, the various forms of expressive behavior either have nothing to do with either means or ends, *e.g.*, style of handwriting, or else they come close to being ends-in-themselves behavior, *e.g.*, singing, sauntering, painting, extemporizing at the piano, etc.[5] This point will be examined more fully in another paper.

[5] In our over-pragmatic culture, the instrumental spirit can overtake even the end-experiences; love ("It's the normal thing to do."); sport ("Good for the digestion."); education ("Raise your pay."); singing ("It's good for chest development."); hobbies ("Relaxation improves sleep."); beautiful weather (". . . good for business."); reading ("I really should keep up with things."); affection ("Do you want your child to be neurotic?"); kindness ("Bread cast upon the waters. . . ."); science ("National defense!"); art (". . . has definitely improved American advertising.").

Coping and consciousness. Expression in its purest forms is unconscious, or at least not fully conscious. We are ordinarily unaware of our style of walking, or standing or smiling or laughing. It is true that we may be made aware of them by moving pictures, phonograph records, caricatures or imitations. But such are apt to be exceptions or at least uncharacteristic. Expressive acts which are conscious are seen as special, unusual or intermediate cases. But coping behavior may be and characteristically is fully conscious. When it is unconscious, *this* is seen as exceptional or unusual.

II. RELEASE AND CATHARSIS; THE INCOMPLETED ACT

There is a special type of behavior which, though essentially expressive, has nevertheless some usefulness to the organism, sometimes even a wished-for usefulness, *e.g.*, what Levy (22) has called release behavior. Perhaps cursing to oneself or similar private expressions of rage are better examples than the more technical ones offered by Levy. Cursing is certainly expressive in being a reflection of the state of the organism. It is not coping behavior in the ordinary sense of being produced in order to gratify a basic need, though it may be "satisfying" in another sense. It seems rather to produce as a by-product a change in the state of the organism itself.

Probably all such release behaviors can be generally defined as keeping the organism more comfortable, *i.e.*, keeping tension-level down, by (a) allowing an uncompleted act to be completed; (b) draining off accumulations of hostility, anxiety, excitement, joy, ecstasy, love or other tension-producing emotions by allowing consummatory motor expression, or (c) permitting simple activity for its own sake of the type indulged in by any healthy organism.

It is very likely that catharsis, as originally defined by Breuer and Freud (7), is in essence a more complex variant of release behavior. This, too, is the *free* (and, in a special sense, satisfying) expression of an impeded, uncompleted act which, like all impeded acts, seems to press for expression. This would seem also to be true for simple confession. Perhaps even full psychoanalytic insight, if we knew enough about it to dissect it carefully, might be found to fit into our series of release or completion phenomena.

It seems to the writer to be here desirable to keep separated from each other those perseverative behaviors which are a coping response to threat and those which are simply and unemotionally tendencies to complete a half-completed act or series of acts. The former have to do with threat to and gratification of basic, partial and/or neurotic needs. They are therefore properly in the jurisdiction of motivation theory. The latter are very probably ideomotor phenomena which in turn are very closely related to such neurological and physiological variables as blood-sugar-level, adrenalin secretion, autonomic arousal and reflex tendencies. Thus, in trying to understand the little boy jumping up and down for (pleasant) excitement, it is best to invoke the principle of motor expression of a physiological state of affairs rather than to refer to his motivational life. The next section deals with perseverations which cope with motivational problems.

III. Repetition Phenomena; Persistent, Unsuccessful Coping; Detoxification

The repetitive nightmare dreams of the traumatic neuroses, the wilder "bad dreams" of the insecure child (or adult), the child's chronic fascination with just that which frightens him most, tics, rituals and other symbolic acts, dissociative acts, and the well-known neurotic "acting out," are all examples of repetitive phenomena which demand special explanation.[6] How important they are can be judged from the fact that Freud felt it necessary because of such phenomena to overhaul some of his most basic theories (12, 13). Several recent writers, for example Fenichel (9, 10), Kubie (20) and Kasanin (16), have pointed to what is probably the solution of this problem. They see these behaviors as repeated efforts, sometimes successful, more often not, to solve an almost insoluble problem. Such behavior may be likened to the hopelessly outclassed but desperate fighter who gets up off the floor again and again, only to be knocked down again and again. In short, they seem to be persistent if almost hopeless efforts at mastery of a problem by the organism. In our terms, therefore, they must be considered to be coping behaviors, or, at any rate, attempts at coping. They thus differ from simple perseveration, catharsis, or release since the latter do no more than complete the uncompleted and resolve the unresolved.

A child overwhelmed by repeated stories in which wolves figure will in some cases tend to come back to the problem again, *e.g.*, in play, conversation, questions asked, making up stories, and in paintings. The child may be said to be detoxifying or desensitizing

[6] We restrict ourselves here to symbolic acts, resisting the temptation to plunge into the fascinating and obviously pertinent problem of symbolism in general. As for dreams, it is obvious that in addition to the type mentioned here, there are also primarily coping dreams (*e.g.*, simple wish fulfillment) and primarily expression dreams (*e.g.*, insecurity dreams, projection dreams). This latter kind of dream should, in theory, be usable as a kind of projective or expressive test for the diagnosis of character-structure.

the problem. This result comes to pass because repetition means familiarization (28), release and catharsis, "working through," ceasing to respond with emergency reactions, slowly building up defenses, trying out various mastery techniques one after the other, discarding useless techniques, practicing successful ones, etc. (36).

We may expect the repetition compulsion to disappear with the evaporation of the determinants which brought the compulsion into existence. But what shall we say about the repetitions which do not disappear? It would appear that in such cases the efforts at mastery failed.

Apparently the insecure human organism cannot accept defeat gracefully. It must keep on trying again and again, useless though this may be. Here we may recall the experiments started by Ovsiankina (33) and Zeigarnik (41) on the perseveration of incompleted tasks, that is to say, unsolved problems. Recent work has shown that this tendency appears only where threat to the personality core is involved, *i.e.*, where failure means loss of safety, self-esteem, prestige or the like. On the basis of these experiments it seems reasonable to add to our phrasing a similar qualification. We may expect permanent repetition, *i.e.*, unsuccessful coping, where a basic need of the personality is threatened and where the organism has no successful way of solving this problem.

The distinction between relatively expressive and relatively coping perseverations not only cuts across a single class of acts, but also enlarges each of the newly divided subclasses. We have seen that the rubric "expressive perseveration" or "simple act-completion" includes not only releases and catharses but also probably motor restlessness (23, 24), expressions of excitement, either pleasant or unpleasant, and ideomotor tendencies in general. It is equally possible (or rather fruitful) to include under the rubric "repetitive coping" such phenomena as unresolved feeling of insult or humiliation, unconscious jealousy and envy, persistent compensation for inferiority feelings, the compulsive and persistent promiscuity of latent homosexuals, and other such vain efforts to remove a threat. We could even suggest that with suitable conceptual modifications the neurosis itself could be so conceived.

Of course it is necessary to remind ourselves that the task of differential diagnosis remains, *i.e.*, is this particular repetitive dream in this particular person expressive or coping, or both? See Murray's list below for further examples.[7]

IV. THE DEFINITION OF NEUROSIS

It would now be universally agreed upon that the classical neurosis as a whole as well as single neurotic symptoms are characteristically coping mechanisms (30). It was one of Freud's greatest contributions to show that these symptoms had functions, aims, purposes, and that they achieved effects of various sorts (primary gains).

But it is also true that many symptoms have been called neurotic which are not truly coping, functional, or purposive behaviors, but which are rather expressive behaviors. It seems to this writer that it would be more fruitful and less confusing to call *only* those

[7] Unconscious needs commonly express themselves in dreams, in visions, in emotional outbursts and unpremeditated acts, in slips of the tongue and pen, in absent-minded gestures, in laughter, in numberless disguised forms fused with acceptable (conscious) needs, in compulsions, in rationalized sentiments, in projections (illusions, delusions and beliefs), and in all symptoms (hysterical conversion symptoms particularly), and in such things as children's games, regressions, doll play, making up stories (TAT), finger painting, man-drawing and fantasy productions (32).

behaviors neurotic which are primarily functional or coping; behaviors which are predominantly expressive ought not to be called "neurotic" but rather by other names (see below).

There is a simple enough test, simple at least in theory, for making the differentiation between symptoms which are truly neurotic, *i.e.*, functional, purposive or coping and symptoms which are primarily expressive. If a neurotic symptom *does* have a function, *does* do a job for the person, then we must assume that the person is better off for having this symptom. If it were possible to rob the patient of a truly neurotic symptom, then, according to theory, he should be hurt in one way or another, *i.e.*, thrown into anxiety or acutely disturbed in some other way. A fair analogy would be ripping away the foundation stone from underneath the house which rests upon it. If the house in reality rests upon it, then it is dangerous to rip it, even though it may be crumbly or rotten or not as good as some other stone.[8]

If, on the other hand, the symptom is not truly functional, if it does not play some vital role, then ripping it away will do no harm, but will only benefit the patient. One of the usual strictures against symptom therapy is based on just this point, *i.e.*, assuming that the symptom which seems to the onlooker to be wholly useless actually plays some important role in the psychic economy of the patient, it therefore ought not to be tampered with until the therapist knows exactly what this role may be.

What is implied here is that while symptom therapy is admittedly dangerous for truly neurotic symptoms, it is not at all dangerous for symptoms which are merely expressive. These latter may be ripped away with no consequence but benefit for the patient. This implies a more important role for symptom therapy than psychoanalysts would now allow (37).

It also helps to teach us that the neurosis has ordinarily been conceived too simply. In any neurotic person one may find both expressive symptoms and coping symptoms. It is as important to differentiate between them as it is to differentiate between prior and subsequent. Thus a feeling of helplessness found in a neurotic person ordinarily gives rise to reactions of various sorts by which the person tries to overcome the feeling of helplessness, or at least to live with it. These reactions are truly functional. But the feeling of helplessness itself is primarily expressive; it does the person no good; he never wished it to be that way. It is for him a primal or given fact to which he can do nothing but react.

V. CATASTROPHIC BREAKDOWN; HOPELESSNESS

Occasionally it happens that all the defensive efforts of the organism fail. This may be either because the dangers which press from the outside are too overwhelming or else because the defensive resources of the organism are too weak.

Goldstein's profound analyses of brain-injured patients (14) demonstrated for the first time a differentiation between coping reactions, however feeble, and the catastrophic breakdown which results when coping is made impossible or useless.

The kind of behavior that ensues can be seen in the phobic who is caught in the situation he is afraid of (21), in reactions to overwhelmingly traumatic

[8] Mekeel (31) has given us a good example of a woman who was hysterically paralyzed and who was told that she was. A few days later she went into complete collapse, but the paralysis disappeared. At the hospital she stayed in collapse. The paralysis never recurred but later she became hysterically blind.

experiences (15), etc. Perhaps it is even better seen in the frantic disorganized behavior of the so-called "neurotic" rats (25). These animals are of course not at all neurotic in any rigorous sense of the term. A neurosis is an organized reaction. Their behavior is quite disorganized.

In addition, another characteristic of catastrophic breakdown is that it is functionless, or purposeless; in other words it is expressive rather than coping. It therefore should not be called neurotic behavior but had best be characterized by some special name such as catastrophic breakdown, disorganized behavior, induced behavior disturbance, etc. (30).

Another example of this kind of expression which should be differentiated from neurotic coping is the deep hopelessness and discouragements sometimes found in people or in monkeys (27) who have been subjected to a long line of disappointments, deprivations and traumata. Such people may come to the point where they simply give up trying, mostly because they seem to see no use in it. If one hopes for nothing, one fights for nothing. There is, for instance, a possibility that the apathy of the simple schizophrenic may be interpreted as an expression of hopelessness, or discouragement, that is to say, as the giving up of coping rather than as any particular form of it. Apathy can certainly be differentiated as a symptom from the violent behavior of the catatonic or the illusions of the paranoid schizophrenic. These would seem to be true coping reactions and would therefore seem to indicate that the paranoid and catatonic schizophrenic are still fighting, and still hoping. In theory as well as in fact we should then expect better prognoses for them.

A similar differentiation with similar consequences may be observed in suicidal people, on the deathbed, and in patients' reactions to their lesser illnesses. Here, too, the giving up of coping efforts influences prognosis markedly.

VI. Psychosomatic Symptoms

Our distinction should be especially useful in the field of psychosomatic medicine. It is in this field that Freud's too-naive determinism has done the most damage. Freud made the mistake of identifying "determined" with "unconsciously motivated" as if there were no other determinants of behavior, *e.g.*, considering all forgetting, all slips of the tongue, all slips of the pen to be determined by unconscious motivations alone. He stigmatized as non-deterministic anyone who pointed to other possible determinants of forgetting, etc. To this day many psychoanalysts can conceive of no explanation other than unconscious motivation. Such a stand need not be crippling in the field of neuroses because in fact practically all neurotic symptoms *do* have unconscious motivation (along with other determinants, of course).

In the psychosomatic field this point of view has created a good deal of unnecessary confusion, for a good many relatively somatic reactions have no goals or functions and are not motivated either consciously or unconsciously. Such reactions as high blood pressure, constipation, gastric ulcers, etc., are more apt to be by-products or epiphenomena of complex chains of psychic and somatic processes. No one has ever, in the beginning at any rate, wished for ulcers, hypertension or coronary attacks (leaving aside for the moment the question of secondary gain). What a person may wish for —hiding passive tendencies from the world, repressing aggressive tendencies, or living up to a certain ego-ideal— may be obtainable only at a somatic

price, but this price is always, of course, unanticipated and certainly not wished for. In other words, such symptoms ordinarily do not have any "primary gain" as the general neurotic symptom does.

An excellent example is seen in the broken bones of Dunbar's accident-prone cases (8). Their carelessness, haste, slipshodness and hobo-like character certainly made broken bones more likely, but these fractures are their fate, not their goal. Such fractures serve no function and do no good.

It is granted that it is possible—even though not probable—that such somatic symptoms as we have mentioned may be produced as a neurotic primary gain. In such a case they had better be labelled for what they are—conversion symptoms, or more broadly, neurotic symptoms. Where somatic symptoms are the "unforeseen somatic price" or epiphenomena of neurotic processes, they had better be labelled with some other title, *e.g.*, physio-neuroses, or as we have suggested, "expressive somatic symptoms." The by-products of a neurotic process should not be confused with that process itself.

The most obviously expressive type of symptom may be mentioned before leaving the subject. These are the symptoms which are expressed or are actually part of a very general organismic state of affairs, *i.e.*, depression, good health, activity, apathy, etc. If a person is depressed, he is depressed all over. Constipation in such a person is clearly not coping, but expression (although it is quite clear that it may be a coping symptom in another patient, *i.e.*, the child who withholds his feces as an act of unconscious hostility to an annoying mother). So, also for loss of appetite or speech in apathy, for good muscular tone in good health, or for jumpiness in the emotionally insecure person.

A recent paper by Sontag (34) may serve to demonstrate the various possible alternative interpretations of a psychosomatic disorder. This is a case report of a woman with severe and disfiguring acne of the face. The original appearance of the condition and its recurrence in three separate episodes coincide with severe emotional stress and conflict over sexual problems. The skin disease came in each of the three episodes at just such a time as to prevent the woman from having sexual contact. It might be that the acne was unconsciously elaborated out of a wish to avoid the sexual problem, and perhaps, as Sontag suggests, as a self-imposed punishment for her wrong doing. In other words, it may have been a purposeful process. It is impossible to be sure of this from the internal evidence, as Sontag himself admits the whole thing may have been a series of coincidences.

But it may also have been one expression of a generalized organismic disturbance involving conflict, stress, anxiety, *i.e.*, it may have been an expressive symptom. This paper by Sontag is unusual in one respect. The author recognizes clearly the basic dilemma in this sort of case, namely, that there are alternative possibilities of explaining the acne as an expressive symptom or as a coping symptom. Most writers with no more data than Sontag have permitted themselves to come to a positive conclusion in one direction or another, *i.e.*, being certain in some cases that it was an expressive by-product, being just as certain in other cases that it was a neurotic symptom.

I can think of no better way to drive home the necessity for caution in attributing purposiveness to what may be coincidence than the following case whose source I have unfortunately not been able to trace. The subject was a psychoanalytic patient, a married man

undergoing severe guilt reaction because of his clandestine sexual relations with a mistress. He also reported a severe skin rash which developed after each visit to his mistress and at no other time. As affairs stand today in psychosomatic medical circles, most practitioners would assume this to be a neurotic reaction, coping because self-punishing. Examination, however, brought to light a much less esoteric explanation. It turned out that the bed of the patient's mistress was infested with bedbugs!

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A FUNDAMENTAL PRINCIPLE OF PERSONALITY MEASUREMENT [1]

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An individual reveals his own personality through any change he makes upon any type of material. This is a useful generalization which I cannot find explicitly stated in the literature on personality. It is not, however, an original idea, for it is implied in the writings of Freud, Allport, Bartlett, Levy, Sears, and others. Freud's *The Psychopathology of Everyday Life* was an elaboration of the notion that the commonplace forgettings and slips of the tongue of everyday life are causally determined by the individual's own personality. In his book *Remembering*, Bartlett's principal finding was that "the material often suffers considerable change in serial reproduction." "Remembering," Bartlett explained, "is an imaginative reconstruction; it is not the re-excitation of innumerable fixed, lifeless, and fragmentary traces" (3). But Bartlett emphasized the change as such rather than what the change represented for the individual making it. Allport, in discussing Bartlett's work, got a little closer to the generalization. He wrote: "Thanks to Bartlett we know how cultural schemata alter our memory traces . . . Edwards has demonstrated that if memory material fails to fit comfortably into an ego-involved frame, it contorts itself until it does so" (2). He came still closer to this generalization in defining the personal document as "any self-revealing record that intentionally or unintentionally yields information regarding the structure, dynamics, and functioning of the author's mental life" (1).

The development of projective tests represents a somewhat different recognition of the extremely wide variety of materials useful as indicators of personality differences. All projective techniques, in fact, assume that an individual's responses to the unstructured test situations will reflect his own personality peculiarities. The clearest statement of this principle was given by Sears in saying that "though the term *projective technique* includes such widely varying methods and mediums as plastic materials, puppet shows, Rorschach Ink Blots, the Thematic Apperception Test, the World Test, and doll play, there is a common assumption underlying the use of all, viz., that the individual by his interpretation or organization of the materials reveals his own thoughts, motives, understandings, and emotions" (4). It is only a small step from Sears' statement to the completely generalized form in which I have expressed the principle: an individual reveals his own personality through any change he makes upon any type of material.

I intend to develop some of the implications of this principle for the construction of personality tests. In attempts to understand an individual's personality, various methods have been used. Examinations of the life history and depth interviewing take advantage of the wide variability of individual experiences but are too unstandardized and time-consuming to serve as personality tests in the ordinary meaning of the word *test*. Questionnaires like

[1] A paper given at the Midwestern Psychological Association, St. Paul, Minnesota, on Friday, May 7, 1948.

the Woodworth and Thurstone inventories and their many imitations have been severely criticized as too highly structured and intellectualistic. The projective techniques have become popular as compromises which retain some of the standardization of the questionnaire and some of the freedom of the interview. But all projective techniques can be criticized as failing to meet the usual statistical criteria of adequate test standardization.

If we examine the meanings of the principal terms included in this generalization, we can arrive at some useful principles to guide us in the future development of tests of personality.

An individual reveals his own personality through any change he makes upon any type of material. The principal terms in this statement are *reveal, personality, change,* and *type of material.* I will define them in reverse order. By *type of material* I mean the test itself, its problems, questions, things to be remembered, arranged, interpreted, or otherwise dealt with. By *change* I mean any interpretation, reorganization, alteration, addition, or omission that the person makes in or to the material presented him. He cannot possibly reproduce it identically unless it is very short. By *personality* I mean all of the motives, emotions, thoughts, attitudes, and their interrelations usually considered under the heading of personality dynamics. By *reveal* I mean that some aspect of the personality is laid open for inspection. But in order to be usefully revealed, several judges or examiners must agree reasonably closely in their interpretation of the change or behavior that the test material has elicited. The judges, in other words, must be able to judge it reliably. The interpretations of the behavior must agree with other evidence concerning the personality. The personality, in short, is not revealed unless it is revealed reliably and validly.

Now what types of material will elicit those changes which most reliably, completely, and accurately reveal the aspects of personality in which we are interested? Several negative answers can be given immediately.

If the material demands a routine response, for example one that has been overlearned in school, differences do not appear; materials such as vocabulary tests are relatively valueless as personality tests.

If different individuals give different responses, but those differences cannot be shown to be consistently related to major personality differences, the materials used are inappropriate; the responses produced lack validity. For example, if we ask a number of people to copy a prose passage, their handwritings will vary greatly. But it has not been demonstrated that handwriting differences bear any significant relations to personality characteristics.

If the changes or responses of a given individual cannot be consistently described or reported by several judges working independently, the materials used are inappropriate, for they fail to measure anything with reliability.

As long as we can conscientiously avoid materials with these three limitations we can give our imaginations complete freedom in choosing suitable materials for personality tests. Doll play, the Rorschach, the Thematic Apperception Test, and other well known projective devices by no means exhaust the possibilities. We could, for example, construct a word association test made up of ambiguous words such as ***ring***. Do we learn anything useful about their personalities from knowing that one subject responds with ***church bells*** and another with *wedding*, while a third says *boxing?* As another possibility we might combine different emotional

statements or statements of areas of possible conflict in too long a paragraph to be remembered. Would personality differences be revealed by a study of the parts omitted and those given relatively different emphasis when the subject attempted to reproduce the original passage?

I do not know the answers to the specific questions I have been asking. The answers would have to be determined experimentally for any new materials just as they ought to be determined experimentally for the projective techniques already in use. Determining the answers experimentally will involve more exact work than has usually characterized the development of personality tests, for this analysis points up the necessity of determining the reliability and the validity of personality tests just as we have grown accustomed to demanding reliability and validity information concerning tests of ability or achievement.

One thing that will surely show up in experiments on different materials is that the nature of the material itself partly determines what the subject can do with it and thus partly determines what aspects of personality he can reveal through it. The change wrought by the individual is some measure of himself, but he brings this change about in the specific material presented to him. The play therapist, for example, may use doll families. The child confronted with these materials feels the necessity of dealing with a doll family. He may be a young genius who prefers a chemistry set, but he has been handed a doll family to play with. Through his responses you can learn something about his relations with his own family. Perhaps you can learn that he admires chemists and feels frustrated. But you cannot learn as much about his unusual knowledge of chemistry or the extent to which it colors his other activities as you could with different tests.

The fact that the test materials limit the range of possible responses is to be expected. That situation obtains in other kinds of testing; an arithmetic test tells little about a subject's vocabulary; a test of mechanical aptitude gives no information on artistic ability. There is no more reason to look for or to expect to find omnibus tests of personality than to expect omnibus tests of aptitude or achievement.

Let me recapitulate: an individual reveals his own personality through any change he makes upon any type of material. This generalization is a logical extension of the thinking behind projective techniques and is found implied in the writings of a number of psychologists interested in personality. Stating the principle in its most generalized form frees us to look for any kind of personality testing material that looks promising. Stating it as an abstract principle also reminds us of the characteristics which any material must possess if a subject's responses to it are to serve as useful indicators of the ways in which his personality differs from the personalities of other individuals. Any material will itself limit the range of possible responses. It is therefore likely that it will be of relatively little value for measuring some aspects of personality, even though it is of great value in measuring others. If personality is to be measured, the changes or behavior which the material evokes must satisfy the usual test criteria of reliability and validity.

The principle which I have been elaborating therefore forces us to think more clearly concerning two aspects of personality measurement.

1. The range of possible test materials is almost infinitely large.

2. Which materials within this large range can be employed depends upon

how satisfactorily they satisfy the well-established statistical criteria of test construction.

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AN INTERPRETATION OF THE EFFECTS OF COMBINATIONS OF STIMULI (PATTERNS) BASED ON CURRENT NEUROPHYSIOLOGY [1]

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When two stimuli in spatial or temporal juxtaposition (*i.e.*, in a "pattern") act upon an organism, their combined effect is not the arithmetical sum of the effects that the individual stimuli would have if presented singly. If the separate stimuli evoke the *same* reaction, though perhaps in unequal strength, the combination will evoke that reaction in a strength that is usually less than the arithmetical sum of that of the separate reactions (see Hull, **11**, pp. 204–225). If the separate stimuli evoke *different* reactions, these too may be modified in strength when the stimuli are presented together; but, in addition, the reaction to the combination may include elements that are not to be found in the reactions to the separate stimuli. Thus, a grey patch surrounded by blue looks yellowish, and surrounded by red it looks greenish. Similarly, as Koffka (**13**) points out, if the two bi-dimensional figures illustrated in Fig. 1 are placed appropriately in apposition, the combination gives the tri-dimensional impression of a cube.

In the field of conditioning an exceedingly illuminating set of experiments involving the differential reinforcement of stimulus compounds and their components has been reported by Woodbury (**31**). This experimenter first trained a dog placed in wooden stock to raise a light wooden bar with its nose, by having his apparatus so arranged that the raising of the bar automatically resulted in the delivery of a pellet of appetizing food into a pan in front of the dog. Later, a wooden shutter was placed in front of the bar to prevent the dog from nosing it except when the experimenter desired it. Now the shutter would be lifted just after the presentation of the stimuli to be conditioned. These stimuli were two buzzer tones, one high-pitched and the other low-pitched. If, when the buzzers were sounded simultaneously, the dog nosed the bar, the food-pellet would be delivered; but when he nosed it after either buzzer had sounded alone, no food would be delivered. Of each 100 trials, 25 employed high buzzer, 25 low buzzer, and 50 both buzzers together. After about 1100 trials the dog had learned practically always to nose the bar at the sound of the combined buzzers, and practically never when either sounded separately. A second dog, who received food only if the buzzers were presented

[1] The writer is indebted to Dr. L. J. Reyna for some important criticisms of an earlier draft of the manuscript.

FIG. 1. Koffka's cube.

separately, and never after their combination, learned, after about 700 trials, almost invariably to nose the bar to the separate buzzers and almost never to the combination. (In another pair of experiments in which the combined buzzers were presented in succession instead of simultaneously, the results were similar.)

Now, to take the experiment in which the bar-raising response was reinforced to the individual buzzers but not to the combination, it is obvious that the ultimate reaction to the combination was not merely a resultant of the reactions that would occur to the separate buzzers. In fact, the combination came to have an inhibitory effect upon the bar-raising that either of its constituents could produce, and it is therefore obvious that there must have been in the response of the nervous system to the combination some distinctive feature upon which differentiation of response could be founded. In other words, the combination as such must be assumed to have had a *special additional effect* on the organism not produced by either of the constituents.

In connection with the above it is necessary to notice a comment recently made by Meehl (26) regarding Woodbury's experiments. He states that there is "no experimental or *a priori* reason to expect the habit strengths of Woodbury's separate stimuli to simply 'add'. . . ." He goes on to suggest that because "simple addition" is not often found in nature, the diverse responses to the combined and the separate stimuli in Woodbury's experiments are only what one would expect. The implication is that no special explanation of the diversity is called for. But, in reasoning in this way, Meehl has overlooked some of the salient facts of the experiments. In the early stages the tendency to raise the bar is equally marked in response to the separate stimuli and to the combination; and there is *afterwards* a gradually developing inhibition of the response to the combination. The problem is: what is there in the organization of the dog that makes this differentiation possible? Of course, food reinforcement accompanies the presentation of the separate stimuli but not the combination. But if the "added together" reactions of the afferent [2] nervous system to a stimulus A and a stimulus B presented separately were exactly the same as its reaction to stimulus (A + B), to reinforce a response to A and B separately would inevitably be to reinforce it to (A + B).

THE ROLE OF THE INTERACTION OF PHYSICAL ENERGIES

If physical energies occurring simultaneously always acted upon and changed each other, then it might be a sufficient explanation of the special effects of simultaneous combinations of stimuli to say that a difference in physical energies leads to activation of different elements of a given sensory system. Simultaneous physical energies do sometimes modify each other. For example, hot water added to cold water gives a product that is intermediate in temperature; and this product applied, say, to the hand may fail to activate a large number of endorgans that would respond to heat or cold.

But such physical interaction appears to be quite unusual. Light waves, for instance, do not interact; and a mixture of light of various colors that gives a visual impression of whiteness is found upon examination to be made up of wave lengths corresponding to the original colors in their original proportions. Physical interaction of sound waves seems also to be unknown; and even the additional sounds produced

[2] The word "afferent" is used here in a broad sense to include all neurones other than motor neurones.

by certain combinations of tones and known as *difference tones* were found by Lewis & Larsen (17) to be due, not to the physical interaction of the sound waves, but to a vibration generated in the ear. In other words it would appear that certain combinations of sound waves set up vibrations in the ear in addition to the vibrations caused by the individual constituent waves. We have, then, an example of an interactive effect taking place in a sense organ.

Such interaction, if it could be assumed to be universal, would provide a simple explanation of the special effects of combinations of stimuli. As it is, it affords at best a partial explanation; for it does not help us to understand the effects of either stimulus combinations that are successive instead of simultaneous or combinations of which the constituents belong to different sensory modalities.

THE GESTALT HYPOTHESIS

The Gestalt writers, who have given much attention to perceptual complexities, have put forward the hypothesis that concrete objects are made up of a variety of organized wholes, and corresponding to each perceived whole there is a physiological process in the nervous system of the perceiving organism. Thus, Köhler (15, p. 46) assumes a "specific relationship" between the physical and physiological orders of events, a relationship of "congruence or isomorphism in their systematic properties." Whatever in the outside world is perceived, therefore, is mirrored in physiological processes in the organism; and, in being mirrored, retains its particular organization. In Köhler's words (15, p. 87), "In the nervous system we have a topographical arrangement preventing confusion and mixture during conduction." In a later work (16), he has this to say regarding the neural processes involved ". . . the neural functions and processes with which the perceptual facts are associated in each case are located in a continuous medium; and the events in one part of this medium influence the events in other regions in a way that depends directly on the properties of both in their relation to each other."

Let it be repeated: the response to the perceived whole is more than (or at least different from) the response to the sum of its parts perceived separately. Köhler recognizes that this must depend upon certain properties of the nervous system. But he makes no attempt to suggest a hypothesis of the mechanism by which this occurs on the basis of our positive knowledge of the nervous system. It merely begs the question to say that the external object is mirrored in physiological processes, that the nervous system possesses a topographical arrangement preventing confusion, or that the neural functions with which the perceptual facts are associated are located in a continuous medium. Nobody can hope to obtain an understanding of the perceptual process if he ignores the firmly established fact that the "total image" an organism gets of anything in its environment is a consequent of impulses that have passed along a multitude of *individually stimulated neurones.* Köhler does, in fact, give the subject of these individual stimulations cursory and somewhat disdainful consideration (15, p. 100) but quite fails to see that the *mechanism* of their synthesis is the crux of the problem of perceptual "wholes." In so far as he discusses a mechanism at all, he does so in terms of vague analogies of the energy exchanges of physics (15, pp. 101–110). Koffka, similarly, employing a more "metaphysical" approach, sidesteps the basic issue (14, pp. 56–67).

HULL'S HYPOTHESIS OF AFFERENT NEURAL INTERACTION

A hypothesis that does take into account the fact that the first response of the nervous system to stimulation consists of the passage of impulses along discrete, individual neurones has been elaborated by Hull (11, pp. 42–44; 12) in the form of the hypothesis of "afferent neural interaction." Hull states (11, p. 43), "It would appear that a given receptor discharge (s_1) is modified by other receptor discharges (s_2 or s_3) on their way to or actually entering the central nervous system at about the same time." The effect of this interaction of discharges is *to change somewhat the course of each.*

Meehl (26) has correctly pointed out that the neurophysiology that underlies this theory of Hull's is vague, but this does not detract from the significance of the fundamental idea that the interaction of impulses passing along individual neurones is the basis of the special effects of combinations of stimuli. Meehl, it appears, has altogether failed to realize this, as evidenced by his comment that Hull's hypothesis "has nothing to recommend it over the already admitted fact that the presentation of another stimulus often makes a difference in the behavior of the organism."[3] It is, of course, obvious that an added stimulus may change behavior; but the point is (with special reference to Woodbury's experiment) that if two stimuli, considered together, always excite the same neural pathways, whether they are presented separately or in combination, it will never be possible to reinforce to the combination a response that differs in kind from a given response which is being reinforced to each of the constituent stimuli separately. Differential reinforcement is possible only if the combination can activate certain *different* pathways, and Hull's hypothesis attempts to explain how this may happen.

Although the idea of neural interaction seems to be essential to any hypothesis that would account for the activation of these additional pathways, Hull seems to have been mistaken in assuming that interaction has this result because it produces changes in the *characteristics of the impulses* (discharge). In accordance with the all-or-none law, the only change that is known to occur (given constant gross physiological conditions) in relation to the *impulses* transmitted by a neurone is a change of frequency, and that is known to be a correlate of the *intensity* of the antecedent stimulus. But if it were supposed that neural interaction were also able to modify frequencies in certain neurones, then, in an experiment like Woodbury's (31) in which two stimuli in combination evoke a response different from that of either alone, the difference in frequency due to interaction would determine the different pathways leading respectively to positive and negative responses. For this to happen, it would have to be the case that the recipient neurones in contiguity with a given neurone are each attuned to a different, but definite and constant, frequency to which alone response can be made. There are two objections to such a theory: it is not supported by any of the known facts of neural transmission; and it is greatly weakened by the fact that a continuous, uniform stimulus energy produces a whole range of impulse frequencies in the nerve fibres which it stimulates (1, pp. 114–117).

[3] A view much like that of Meehl has been expressed by E. R. Hilgard in his book, *Theories of Learning* (New York: Appleton-Century, 1948).

A NEW NEUROPHYSIOLOGICAL HYPOTHESIS OF NEURAL INTERACTION

It is quite possible to understand the special effects of stimulus compounds on the basis of the findings of present-day neurophysiology. A hypothesis derived from a consideration of known mechanisms is outlined below. The following are the neurophysiological facts upon which this hypothesis depends.

A synapse is the functional junction between one neurone and another. One-way transmission across a synapse is the rule. Several pre-synaptic neurones may converge to form synapses with one post-synaptic neurone, and the various synapses are then said to make up a synaptic zone. A number of factors are known to determine whether or not a synapse will transmit an impulse from a pre-synaptic neurone to a post-synaptic neurone. Chemical agents like strychnine increase synaptic excitability (29), and agents like nembutal reduce it (7). Excitability is also reduced immediately after an impulse has been conducted by the post-synaptic neurone, when, for a short period, which varies in length from 0.5 to 3.0 milliseconds, the neurone is absolutely refractory, and will not respond to any further impulse (4, 5, 20). For a period of about 15 milliseconds after this absolutely refractory period, there is a relatively refractory period, or period of subnormality (4, 5, 21). During subnormality the neurone is refractory to weak stimulus energies only, and will transmit an impulse if the stimulus energy at the synapse is sufficiently strengthened. The impulse that is then transmitted is of normal amplitude, in accordance with the all-or-none principle.

If, at any time other than the absolutely refractory period, an impulse too weak to evoke a response in the post-synaptic neurone is followed after 0.1 to 0.5 millisecond by a second inadequate impulse along the same neurone, the effects of the two impulses may summate so as to excite the post-synaptic neurone (9). This is known as *temporal summation*. In like manner, the summation of the effects of impulses reaching a synaptic zone from two or more neurones may result in excitation of the post-synaptic neurone, though the separate impulses would have been ineffective. This phenomenon, known as *spatial summation*, has been demonstrated by Denny-Brown and Sherrington (3), Eccles and Sherrington (8), Bremer (2) and Lorente de No (22, 23). Lorente de No (22) has found, in eye-muscle nuclei, that for spatial summation to occur the interval between the impulses must not exceed 0.1 to 0.2 millisecond; and that summation is maximal when the impulses arrive simultaneously. Eccles (6) has shown that in the autonomic nervous system the interval may amount to 15 milliseconds or more.

Summation of the effects of impulses more widely-spaced in time seems to be due to the activity of those chains of internuncial neurones that are arranged in circular formations in such a way that if one part of a chain is stimulated, a circus movement is set up in the chain. And since at least some of the neurones comprising the chain also give off impulses at synapses *outside* the chain, impulses will be delivered at these synapses for as long as the circus movement continues. Impulses that are supraliminal will evoke responses in the contiguous neurone at the synapse. But if the impulses are subliminal and any one of them reaches the synapse synchronously with a subliminal impulse from elsewhere, a supraliminal effect may result from their

spatial summation. The evidence in support of the above statement regarding the activity of internuncial neurones has been reviewed in two lucid articles by Lorente de No (24, 25).

Summation is *not the invariable result* when impulses arrive at a synaptic zone simultaneously from different neurones. Impulses delivered by certain neurones appear to have an inhibitory effect at the synaptic zone on the impulses that other neurones deliver. Indications of this were obtained by Eccles and Sherrington (10) some years ago. More direct demonstrations of this kind of inhibition (which is known as *direct inhibition*) have lately been provided by Lloyd (18, 19) and Renshaw (27, 28).

The neurophysiological knowledge outlined above may be applied as follows to the findings of Woodbury's experiment. When the high buzzer in this experiment produces an impulse in an afferent neurone, responses follow in neurones which are contiguous with this afferent neurone and stimulable by impulses from it. Each responding neurone in turn stimulates stimulable neurones contiguous with it. But *in many contiguous neurones no response occurs:* they are not stimulable by these impulses. The same situation exists in respect of the responses to the low buzzer. Now, suppose that certain of the non-responding neurones are contiguous with both high-buzzer-stimulated and low-buzzer-stimulated neurones. Then it is easy to see that if impulses arrive from both sources simultaneously, the threshold of some of the neurones previously not responding will be exceeded, *so that they will now respond* (due to spatial summation; see Fig. 2). So rich is the mosaic of neural interconnections (particularly in the human being) that it is not difficult to believe that in the combination of almost any two stimuli at least some neurones will be stimulated that would not be stimulated consequent to either of the stimuli singly.

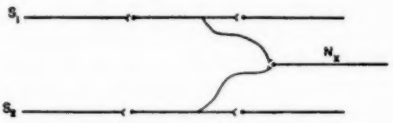

FIG. 2. Spatial summation as the basis of the special effects of compound stimulation. The neurone n_x is activated only when impulses are arriving from *both* S_1 and S_2.

In general terms, then, we may state that *when two stimuli act in combination, they activate not only the neurones that each acting alone would have activated but also certain other neurones.* It is to be concluded that the stimulation of these additional neurones makes such "wholes" as the composite shape in Fig. 1 more than the sum of its parts, and makes possible the differential conditioning in Woodbury's experiment. At the same time, it is well to bear in mind the possibility that due to the operation of direct inhibition (v.s.) at other synaptic zones, certain neurones that respond to one or other separate stimulus will not respond to the combination.

A detailed neurophysiological hypothesis explaining the differential conditioning in Woodbury's experiment has been presented elsewhere (30).

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HILGARD'S CRITIQUE OF GUTHRIE

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Hilgard's recent book, *Theories of Learning* (9), is very critical of Guthrie's theory of conditioning by contiguity. In his chapter on Guthrie, Hilgard sprinkles criticisms through his exposition of the theory and in a section at the end headed "estimate of Guthrie's position" he presents a compact series of objections, making it clear that his estimate is primarily negative. A likely effect of such strongly negative criticism in an introduction to learning theories is to discourage further examination of the theory by introductory readers. This would be unfortunate if only because Guthrie does represent a distinct point of view in a controversy which is far from settled. In addition, however, Hilgard has failed to make a valid case against the theory, and the purpose of the present article is to call attention to important weaknesses in his critique.

A paraphrased list of Hilgard's strongest criticisms follows, with comments on the seriousness or validity of each; comments on what are thought to be minor criticisms by Hilgard are not included. All of the writer's comments refer to Guthrie's own position, not to extensions of it made by others.

(1) *The strongest supporting evidence for the theory is derived from a misleading learning situation.* In his example of a study suggested by Guthrie's theory—the Guthrie and Horton study (5)—Hilgard discounts the findings on grounds that seem very questionable. The purpose of the Guthrie and Horton study was to test the Guthrian prediction that what is learned by a cat in a problem-box tends to be a stereotyped movement—usually the one that obtains the first release from the problem-box—rather than a symbolic representation that would entail a great variety of responses so long as the door gets opened. While granting that the degree of stereotypy obtained was impressive, Hilgard suggests that the results fail to represent most of the learning which took place and that they are the outcome of an unrepresentative learning situation.

Thus the habit of operating the pole to open the door of the problem-box is set aside as a trivial aspect of the task:

> "Most of the change in behavior took place in the neglected early trials in which the cats learned to find their way out of the box through the glass door. There was, to be sure, the supplementary learning to use the pole to open the door" (9, p. 70).

It is not at all clear why the very habit under investigation may properly be called "supplementary learning," nor is it clear, even considering the total learning task from beginning to end, why the three preliminary trials in which the animal is allowed to walk out of the *visibly open* door display "most of the change in behavior." Rather, the impressive change in behavior is the change from the frustrated wandering, climbing against the walls, and pawing at the door—which characterize the first trial with the door closed and the release mechanism functional—to the smooth, relaxed, and immediate operation of the pole on later trials.

It is also not clear why Hilgard concludes that the stereotyping exhibited is not typical of learning. He argues (9, pp. 68–70) that the Guthrie and Horton problem-box presents an "easy" yet "baffling" task to the cat and he

draws on Hamilton's studies (7) as evidence that "baffling" tasks are especially conducive to stereotyping. This argument lacks cogency on a number of counts. For one, the "stereotyping" in Hamilton's studies consisted of useless repetition of an *unsuccessful* response in a *difficult* task, which repetition was fostered by the *emotional upset* of frustrated effort. It is difficult to see how such a finding can be used to explain the Guthrie and Horton result of the repetition of a *successful* response in an *easy* task which is accompanied by *no emotional upset* once the animal acquires a correct response. Moreover, Hamilton called attention only to the repetition of useless movements, later called "persistent non-adjustive responses" (8), and did not show that stereotyping was absent in non-baffling situations. On the contrary, he describes a "baffling" situation (7, p. 21) as one in which the mammalian "is unable to discover and stereotype" an invariably successful response, which implies both that successful responses are stereotyped in non-baffling situations and that the Guthrie and Horton problem—in which the animal *is* able to "discover and stereotype" an invariably successful response—does not fit the specifications of "baffling." Hilgard carries the analogy to Hamilton's studies only by using Hamilton's term, "baffling," with a very different meaning, which in the present instance is that the cause and effect relation between the cat's movement and the opening of the door is not made "clear" to the cat.

(2) *Guthrie's interpretation of the action of reward is refuted by Thorndike's spread of effect.* Hilgard states (9, p. 74) that Thorndike's spread of effect "refutes the theory that reward works by taking the organism out of the situation." This is a surprising objection to Guthrie's theory in view of Hilgard's handling of the spread of effect in his critique of Thorndike. In the chapter on Thorndike Hilgard practically disproves Thorndike's interpretation, mustering so much evidence against the interpretation that he headed a section (9, pp. 44–45), "Is a spread of effect demonstrable?" The reinstatement of the interpretation in the different context of making a case against Guthrie therefore seems out of order. Furthermore Hilgard omits, in making this criticism of Guthrie, an experiment (11) which refuted Thorndike's interpretation of the spread of effect and indicated that the phenomenon was derivable from pure contiguity. While not a direct test of Guthrie's theory, this experiment was suggested by the fact that Thorndike's spread appeared inconsistent with the simple contiguity principle; if Guthrie were correct the Thorndike finding had to be an artifact predictable from contiguity and related principles. The experiment revealed an artifact which very likely accounts for the entire phenomenon, in terms consistent with Guthrie's position. Hilgard includes this study as evidence against "Thorndike's connectionism" but omits it in his critique of "Guthrie's contiguous conditioning."

(3) *The effects of punishment are inadequately handled.* The following statement is practically all Hilgard gives in the way of an exposition of Guthrie's interpretation of the effects of punishment:

"Guthrie describes the results of punishment as he does those of reward, neglecting to consider the differences between the reward and punishment situations. To say that the child does what he last did in the punished situation is not to account for the efficacy of the punishment but for its lack of efficacy. It is all right, in the case of reward, to do what you last did; if, when you have been punished, you do what you last did, you are simply in for more

punishment. Guthrie's anecdotal writing permits him to gloss over a special problem such as that created by punishment" (9, p. 62).

This is a very misleading statement of Guthrie's position on punishment. Guthrie's own statement in his chapter on "Reward and Punishment" (2) is to the effect that:

(a) Punishment works only if the last response to the punished situation is incompatible with the response that brought on the punishment.

(b) Punishment works only if the cues present when the incompatible response is performed are present when the punished response is performed

(c) Punishment that produces only emotional excitement will tend to fixate the punished response.

Quoting from Guthrie:

"Punishment achieves its effects . . . by forcing the animal or the child to do something different and thus establishing inhibitory conditioning of unwanted habits. Punishment is effective *only in the presence of cues for the bad habit.* . . . Furthermore, when the effect of punishment is only emotional excitement, punishment facilitates the stereotyping of the undesired habit" (2, pp. 160–161).

The three foregoing principles are hardly describable as "glossing over" the problem of punishment; instead they provide a basis for fairly detailed predictions of the effects of punishment. Also it is obvious that Hilgard's statement calls attention to the wrong "last" response. The punishment situation begins with the onset of punishment, and the last response is the one correlated with the termination of punishment, not the one that produces the punishment. What the child last does when "punished" by a hot radiator is to withdraw its hand, not to reach for the radiator. Since withdrawal is incompatible with reaching, and the cues for reaching are still present, the child is *not* "in for more punishment" according to Guthrie.

Guthrie also comes in for a brief criticism on punishment in another chapter. In his chapter on Hull's system Hilgard objects both to Guthrie's and to Hull's interpretations of punishment, which he refutes as follows:

"If an animal runs down an alley, meets a charged grid, and leaps ahead from it to safety, the leap is associated with need-reduction (pain alleviation) so that, by strict reinforcement theory, the next time the animal should do what it last did, only perhaps more intensely. It should run down the alley faster, touch the grid and make a jump ahead to safety. (This, by the way, should be Guthrie's prediction also, since leaping from the grid prevented any unlearning of the last bit of behavior to the grid.) Rats do not do this. They slow up in the alley, and try not to approach the grid" (9, p. 108).

However, Hilgard provides no references to back up this confident statement as to what rats do. The fact is that there is no supporting experimental evidence for the statement in its unqualified form. On the contrary, rats often behave exactly in the manner derived by Hilgard from Guthrie and Hull. In attempting to use the obstruction-box method of measuring drives the writer was surprised to discover that some rats learned the habit of dashing out of the experimenter's hand, taking the shock, and ignoring the lure on the far side of the grill because of the emotional effects of the shock. Increasing the shock in such cases only made the animal more vigorous in running into the shock. A critical factor appeared to be whether the animal initially escaped shock by running forward or by backing, exactly as predicted by either Guthrie or Hull but not as predicted by Hilgard.[1] Similar ob-

[1] With a different rodent (the guinea pig) and an activity wheel rather than a runway,

servations have been made by others. Mowrer (12) reports a comparable observation by J. S. Brown. Compare Hilgard's statement as to what rats do with Mowrer's report:

"If, after a 'conditioned' response of the kind just described [escape from shock by running forward] is well established, a small section of the floor grill, at the far end of the alley, is permanently electrified so that in the process of getting from the starting point to the safety compartment, the rat must always receive at least a momentary shock, the running response does not extinguish!" (12, p. 133).

A recent experiment by Gwinn (6) demonstrates that animals which have escaped shock by running forward take much longer to extinguish in running forward if they run into a short charged area of the grill than if all shock is eliminated. However, Gwinn's animals did eventually extinguish, indicating that his "vicious circle" was not as stable as the one described by Mowrer and that the most obvious "last response" is not enough to explain all of the results. But this does not mean that a Guthrian analysis of Gwinn's findings could not be made and proven correct by new experiments. The important fact is that the unexpected, maladaptive result indicated by Guthrie's position but denied by Hilgard is at least in part borne out, indicating Guthrie is pointing to important factors even if one can find situations in which additional factors such as anticipatory "cringing" or "freezing" in response to anticipatory fear play a role not covered in Guthrie's writings.

the writer also found that whether the animal leaped ahead or stopped when briefly shocked in the midst of a conditioned run to tone determined whether strength of conditioned running increased or decreased, respectively, on the next following trial. See: Sheffield, F. D., Avoidance training and the contiguity principle, *J. comp. Psychol.*, 1948, 41, 165–177.

(4) *The theory does not adequately deal with the detailed stimulus and response units which it stresses as important.* Hilgard asserts that there is a discrepancy between the detailed response units implied by Guthrie's theory and the gross response units involved in Guthrie's writings.

"It is a curious thing that a theory which stresses observable minutiae (chiefly proprioceptive stimuli precisely coincident with specific muscular response) should gain its support chiefly from the grossest of observations, primarily anecdotal" (9, p. 73).

Hilgard even goes so far as to suggest that Guthrie has no intention of being taken seriously in his emphasis on detailed stimulus and response relationships and instead seeks only, to quote Hilgard, "a convenient way of talking about things . . . without seeming to contradict the system" (9, p. 63).

In evaluating this criticism it should be noted that the statement of Guthrie's associative principle selected by Hilgard is an unfortunate version which used "movement" as the response term. This version by Guthrie not only indicated a degree of specificity of response which is not a necessary aspect of the theory but also inadvertently excluded conditioned salivation (all of Pavlov's work) and numerous other conditioned responses based on response units not properly described as "movements." Guthrie's first formulation (1) and more recent formulations (*e.g.*, 4) use "response" instead of "movement" and do not commit the theory to any particular unit of response.

Furthermore it should be noted that Guthrie, in an article (3) not cited by Hilgard, recognized the issue Hilgard raises and presented his position on the topic. Guthrie's argument is that there are some phenomena of learning which can be understood only by taking specificity of response into account—at a

molecular level it is the activity of effectors which is predicted from associative learning principles. At the same time Guthrie points out that the prediction of the effects of learning exclusively in terms of detailed effector processes would be highly impractical:

"There is little use for the prediction of movements alone, and there exists almost no vocabulary for their description. To try to describe behavior by naming the muscles in use and the degrees and order of their contraction would be absurd" (3, p. 127).

Furthermore, according to Guthrie there are cases in which a very molar response may properly be described according to its *outcome* rather than in terms of the effectors involved and can be dealt with as a response unit in associative terms. Quoting from Guthrie:

"In a given individual the capacity for an act, the ability to achieve its defining end result, will depend on previous practice. . . . In describing the abilities of a child we are correct in naming certain acts. He can now tie his own shoes, button his own garments (except for unaccustomed arrangements of the buttons), wash his own hands, ride his tricycle. . . . And when these abilities are once established by adequate practice in varied situations *we may very properly deal with them as acts in associative terms.* Finishing our work (which describes a whole class of situations) becomes the cue for going home (which is also a whole class of movements)" (3, p. 145).

Thus one of the problems in Guthrie's system as well as others is the determination of what constitutes a response unit, but the system does not need to bury itself in detailed specificity of stimulus and response. Moreover, as indicated in the foregoing quotation from Guthrie, the theory helps to determine when the response must be dealt with as a specific effector pattern and when it can properly be described in terms of its effect on the environment.

(5) *The apparent practicality of the theory is illusory.* It is not clear whether Hilgard regards the alleged discrepancy between the specificity of Guthrie's principles and the grossness of Guthrie's supporting evidence as a genuine weakness of the system or whether he raises the issue only to discount the evident practicality in Guthrie's writing as not being an inherent aspect of his theory. According to Hilgard, Guthrie's writings are so closely in touch with life and so full of good advice that one might get the false impression that Guthrie's theory is superior to others in this respect.

"Close examination shows, however, that this appeal of practicality and simplicity is gained by the avoiding of responsibility for the very detail upon which the system logically rests, that is, the detail of precise stimulus and response relationships, and the precise movements out of which behavior is composed" (9, p. 74).

Hilgard's explanation of the practicality of Guthrie's writings is simply that Guthrie is a wise man:

"The advice which he gives about learning is generally good advice, for Guthrie is a wise person. But the advice is seldom critically related to the theory, and could as well be derived from the theories of his opponents" (9, p. 74).

This easy resolution of the conflict between Hilgard's evidently high estimate of the practicality of Guthrie's advice and his low estimate of the utility of Guthrie's theory is unconvincing to the writer. Why should the attribute of wisdom show up in the advice but not be reflected in the theory? Moreover, the writer does not agree with Hilgard's implication that the various learning theories are so identical in their deductions—or so loosely stated—that the same advice over a

large range of learning situations "could as well be derived" from each of the theories. It may be helpful at this point to provide a concrete illustration in which Guthrie's advice is not derivable from one of the opposing theories. The illustration also demonstrates a case in which Guthrie's advice is "critically related to his theory." Further, the illustration demonstrates the utility of the system in suggesting concrete experiments, a property allegedly lacking according to Hilgard, who claims "it is not that kind of theory."

The illustration is based on Guthrie's well known advices for breaking an undesirable habit. The methods proposed are described in Guthrie's major presentation of his theory (2) and are presented in Hilgard's book (9, p. 60). All of the methods follow as derivations from Guthrie's interpretation of extinction, which is that if the conditioned stimulus is presented and some other behavior than the conditioned response occurs, conditioned inhibition will alienate the conditioned stimulus from the conditioned response. One of the methods, sometimes called the "toleration" or "crowding-the-threshold" method, consists in presenting such a weak or generalized form of the conditioned stimulus that it is insufficient to arouse the conditioned response. The stimulus pattern is successively made more and more like the original conditioned stimulus, continually "crowding" the subject's threshold for the response but never more so than he will "tolerate," that is, never allowing the conditioned response to occur. The subject "does something else" at each point in the process until finally what had been a well established conditioned stimulus can be presented without evoking the response—and without the subject's ever having given a single response during extinction.

Whereas this advice on breaking habits follows directly from Guthrie's theory, it is in conflict with Hull's theory (10), which says that the conditioned response must *occur* without reinforcement in order to lay down the increments of inhibition that eventually give rise to extinction. This conflict in theories has considerable scope because of its possible relation to clinical practice: how should we best desensitize an anxious patient, by graded exposure to his sources of insecurity or by forcing him directly into the provoking situation and letting him respond with anxiety until he is extinguished? Clinical practice generally favors the Guthrian implication but is open to the suspicion that clinicians favor the toleration method because they would antagonize their patients if they tried the Hullian approach, which may be the more effective one.

Obviously this theoretical conflict could be made the basis for an experimental program. Without going into detail it would be significant merely to demonstrate that Guthrie's toleration method works—that a conditioned response can be extinguished, without ever evoking the response, by gradually "crowding the threshold." To further show, as implied by Guthrie's theory, that this would require fewer trials than direct exposure to the conditioned stimulus would be even more significant. If either implication were demonstrated, some revision in Hull's version of the extinction process would be indicated; if they could not be, some revisions in Guthrie's theory would be indicated.

(6) *Objections to the theorist.* A good deal of the negative impression of Guthrie's theory created by Hilgard derives from the numerous critical references to Guthrie's manner of presentation and alleged irresponsibility in substantiating his claims. Some of these references are phrased as if objections

to the theory, as when Hilgard speaks of the theory's "assured answers" or its "casualness." Others are direct criticisms of the theorist, as when Guthrie is criticized for handling concepts like insight in a "derisive manner," for use of "ridicule" in discussing other authors, for "cavalier handling" of alternative theories, for giving an "impression of immutability" inappropriate in a young science, for "avoiding of responsibility" with respect to the predicting of the details of learned behavior, and so forth. It should be clear that such objections by Hilgard to the *theorist*, while they are responsible for much of the overall low opinion of the theory conveyed, are not criticisms of the theory *per se* and should not be mistaken for weaknesses of the system. Hilgard almost makes this point himself in his concluding paragraph of the chapter, in which he softens some of the preceding objections by noting that Guthrie's method of arguing his case is not necessarily a consequence of the theory. He also indicates in his concluding paragraph more confidence in the basic ideas involved in contiguity theory than found in his preceding criticisms by granting that the theory might possibly be extended and amended by others into a "serious rival of other carefully worked out theories."

The writer agrees with Hilgard that Guthrie's contiguity theory can profit from future extension and amendment but does not agree with the implication that the theory is less carefully worked out or a less serious rival than most other current theories. Certainly it is as carefully worked out as the Gestalt position, which is more favorably reviewed by Hilgard but which is so incompletely developed that his presentation of its "fullest and most systematic" treatment (Koffka's) consists of a description of what the theory *might have been like* if Koffka "had been more systematic about it" (p. 182, ff.). Moreover, Guthrie has elected to expand his theory in terms of practical implications rather than laboratory experiments, and *none* of the others described by Hilgard is as carefully worked out in this respect. Guthrie's informal writing, practical emphasis, and anecdotal illustrations often give a misleading impression of simplicity and looseness of theoretical structure; actually the theory is closely interwoven and subject to considerable specific qualitative ramification.

Critics and reviewers of Guthrie's theory tend to concentrate on his most radical assumptions and to ignore the details of interpretation that make the system useful. Thus Guthrie's assumption of one trial learning is rarely overlooked, but the related interpretation that the form of the acquisition curve is a function of the variability of the external and internal environment is rarely noted despite its practical and theoretical importance and its experimental implications. The postulate of one trial learning may well represent a pure case that cannot be directly tested, or it may even eventually be disproven for some organisms or some responses. But the basic derivation is that acquisition is more rapid the more constant the cues, and this derivation does not depend at all on the assumption that the limiting case of complete acquisition in one trial actually holds. As with many of Guthrie's interpretations, the derivation depends only on the factor of contiguity, the factor of generalization decrement, and knowledge of the stimulus patterns actually present. Derivations of this sort comprise much of Guthrie's theoretical system and will tolerate considerable change in assumptions about basic process so long as learning theory continues to utilize the almost universally accepted factor of contiguity, which other theo-

rists include along with additional factors. To this extent Guthrie's predictions are often derivable from other theories, as claimed by Hilgard, although Guthrie has so far been the chief contributor of such derivations.

One of Guthrie's main contributions has been to show that what other theorists treat as basic properties of the learning process (*e.g.*, the shape of the acquisition curve) actually can be deduced from simpler principles by careful consideration of the implications of the contiguity factor. Whether such deductions are sufficient is a matter for future research, and the burden of proof is on those who believe Guthrie's position is insufficient. Hilgard's critique does not alter this situation because he does not present any experimental evidence which provides an exception to the theory.

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REINFORCEMENT AND LEARNING: THE PROCESS OF SENSORY INTEGRATION

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Introduction

When Pavlov and Thorndike began their investigations of the learning process, they introduced into psychology two methods of experimentation from which there emerged two diverse conceptions of the mechanisms underlying modifiability. The Pavlovian situation focused attention on the development of stimulus-equivalence (32). The classical conditioning experiment begins with an established stimulus-response relationship, s_1–r_1, and a second stimulus, s_2, which does not elicit r_1. After the repeated presentation of s_2 in conjunction with s_1, s_2 presented alone becomes capable of eliciting r_1. If, after the conditioned relationship has been developed, s_2 is repeatedly presented without s_1, it loses the newly acquired functional property, and extinction is said to have taken place. Observations of this sort led Pavlov to the conclusion that contiguity of stimulation is the essential factor in conditioning which he regarded as a process of afferent modification rather than as a process of stimulus-response connection. Contiguous stimulation, he thought, modifies the cortex in such a way that the excitation aroused by the conditioned stimulus subsequently spreads to the afferent area ordinarily activated by the unconditioned stimulus, and thus *indirectly* elicits the unconditioned response. In the Pavlovian experiments a few rather limited responses were selected as indicators of the course of conditioning, and general activity was restricted by elaborate harness. Under these circumstances Pavlov did not have much opportunity to observe qualitative variations in response and so devoted little consideration to response modification in his learning theory.

Thorndike's investigations of trial and error learning in a sense represented the other side of the behavioral coin. While Pavlov's attention was fixed upon afferent relationships, Thorndike was almost exclusively concerned with action (39). In the problem box, variability of response, the occurrence of an appropriate response, and the fixation of that response are the three events upon which observation is focused (1). An animal, placed in the box, displays a sequence of responses which is terminated by the action which permits exit from the box. In successive trials variability of behavior is diminished, and the response of the animal tends more and more to be restricted to those aspects of the terminal action pattern which are prerequisite to escape from confinement. In order to account for the selection of response appropriate to given motivating conditions and the disappearance of irrelevant responses, Thorndike introduced a primitive need-reduction theory, the now classical law of effect.

The two principles—contiguity and effect—are by no means mutually contradictory, and for some time they were implicitly accepted as comple-

mentary postulates which made it possible to explain, although in an incomplete way, many of the characteristic phenomena of animal learning. However, even the implied conception of qualitatively distinct learning processes did not long remain unchallenged. Careful observation soon revealed that progressive modification of response occurred in the Pavlovian situation as well as in the problem box. Although in the early stages of conditioning the conditioned and unconditioned responses closely resembled each other, both responses often appeared to undergo marked alteration, and sometimes in different directions. The concept of stimulus substitution clearly could not account for such results. This difficulty was emphasized by the development of many hybrid learning situations which were intermediate between the classical types in that they retained the Pavlov frame while providing the animal with greater opportunities for adaptive modification of response. At the same time the problem box itself was often complicated by the addition of signal properties. So situationally rooted were the earlier conceptions that the blurring of situational distinctions produced a blurring of conceptual distinctions, and the stage was set for the emergence of a theory which would deal with all of the data in terms of a common underlying principle. The possibility that both of the processes highlighted by the classical investigations might operate concurrently under any given set of conditions was not seriously considered. Instead, in accordance with the facile Watsonian formula, all learning was reduced to the formation and strengthening of stimulus-response connections, and although "conditioning" became the preferred designation of the process, something like the Thorndikian conception of effect came to be generally accepted as the connecting principle.

The culmination of this theoretical trend is to be found in the position of Hull (21) who has worked for many years to give it systematic expression. His well-known fourth postulate reduces all learning to the formation and strengthening of stimulus-response connections through the agency of need-reduction. After considering the relation between conditioning and selective learning, Hull suggests "that the differences between the two forms of learning are superficial in nature; *i.e.*, that they do not involve fundamentally different principles or laws, but only differences in the *conditions* under which the (single) principle operates." True, the conditioning experiment may involve the "setting up of a receptor-effector connection *de novo*" (21, p. 78). While "Simple selective learning may involve the mere strengthening of receptor-effector connections already of superthreshold strength before the beginning of the experiment" (21, pp. 77). But in every instance contiguous need-reduction is responsible for the increment in habit strength. Hull points out that the Pavlovian conception cannot account for the selective modification of response in a given stimulus situation, the phenomenon with which the effect principle was designed to deal. He maintains, however, that stimulus substitution ("the setting up of a receptor-effector connection *de novo*") can be regarded as a special case of effect learning if one assumes that the unconditioned stimulus not only elicits the response to be conditioned but constitutes as well a need-reducing "state of affairs." One has only to look for need-reduction (primary or secondary) and it is readily found. In the classical salivation experiment food presented to the animal "reduces" hunger, while in flexion experiments the termination of

shock "reduces" the need established by its onset. Pavlov's "mistaken" emphasis on the *mere occurrence* of the unconditioned stimulus is attributed to the limited nature of the situation that he employed.

Hull's system has proved to be very attractive because of its presumably parsimonious treatment of a wide range of diverse phenomena. One must recognize, however, that the more parsimonious of the two theories is to be preferred only if it can account for available data at least as well as can its competitor. For this reason, as Hilgard and Marquis have pointed out, "there is no logical necessity for reducing all instances of learning to a single explanatory principle" (20, p. 99). Further, one must distinguish between parsimony in verbalization and parsimony in assumption. It may be argued that Hull's need-reduction formula requires a multiplicity of informal supporting assumptions and that it is therefore only apparently parsimonious.

The contemporary tendency to break down the classical distinction between conditioning and selective learning has not been without its critics (10, 20, 23, 31, 34). However, the clearest development of an alternative position—which constitutes an explicit statement of the potentialities present in the earlier dual-process theories—has been provided by Maier and Schneirla (31). On the basis of a careful analysis of representative conditioning experiments, these writers contend that it is necessary to postulate two distinct processes if the data are to be properly understood. The first is a process of *sensory integration* ("sensory-sensory conditioning") in which one stimulus, as a result of contiguous presentation with a second, gradually acquires the functional properties of the second. In addition, there is a process of *selection* in which the organism comes to make those responses which are qualitatively and functionally dependent upon the prevalent motivating conditions. In the first case a relation between afferent processes is established—there is an "extension or qualitative enlargement of the pattern of stimulation which will elicit the critical response" (31, p. 132)—and its essential condition is contiguity of stimulation. In the second case there is a progressive *modification of response* to given stimulating conditions under the influence of reward and punishment.

It should be noted carefully that the two processes are not situationally defined; under certain experimental conditions both must be assumed to occur. For example, Maier and Schneirla account for the development of a conditioned avoidance response in the following terms: In the first phase of the experiment the conditioned stimulus is paired with the shock to the leg and as a result acquires the property of eliciting flexion. Here a previously ineffective stimulus has been transformed into an effective one, and contiguity of effective and ineffective stimuli has been the essential condition. Once the conditioned stimulus becomes capable of eliciting the response, however, selective learning may supervene. The response may now become progressively more specific in a manner which depends upon the conditions under which shock is administered—*e.g.*, the amplitude of flexion may decrease to the minimum necessary to avoid the grill. It should be clear, therefore, that Maier and Schneirla do not deny that selective learning may go on in the conditioning situation, nor do they deny that the unconditioned stimulus—food or shock—may play an important motivational role. They do maintain, however, that the development of stimulus equivalence which is observed in conditioning is the result of a qualitatively

distinct process which depends primarily on contiguous stimulation. It may also be well to point out that Maier and Schneirla did not undertake an explicit analysis of the selection process. Instead they devoted their main efforts to demonstrating the need for a concept of sensory integration and to analyzing the qualitative differences between this process and a selective process in their consequences for learning.

Is there then a process of afferent modification which is brought about by contiguity of stimulation and which can occur independently of need-reduction? Replying from the point of view of Hull's theory, Kendler and Underwood (25) have recently attempted to demonstrate that the need-reduction principle is adequate to account for all the phenomena of learning, and consequently that the concept of sensory integration in conditioning is superfluous. Unfortunately, however, the Kendler and Underwood paper serves to confuse rather than to clarify the issues. It is our opinion that there is a substantial body of evidence which these writers, like Hull, either entirely ignore or treat in a superficial and misleading manner, and that these data clearly indicate the existence of a process of sensory integration.

SENSORY INTEGRATION IN CONDITIONING

Evidence in support of our position may be marshalled under three main headings. First, an examination of the phenomena of stimulus-substitution reveals that attributing need-reducing properties to the unconditioned stimulus does not fully account for the behavioral events in conditioning. Second, the investigations of sensory preconditioning present positive support for the contiguity principle. Finally, researches on the nature of the neural mechanisms involved in conditioning have crucial implications for the differentiation of sensory integration from response modification. Each of these lines of inquiry will be explored in turn.

1. *The unconditioned stimulus as reinforcing agent.* To account for any instance of conditioning in terms of Hull's theory, one must be able to demonstrate (a) the contiguous occurrence of the conditioned stimulus and the response which it is to elicit, and (b) need-reduction, either primary or secondary, which follows closely upon the stimulus-response conjunction in question. Most conditioning situations, although not all, provide the temporal contiguity of conditioned stimulus and response, but it is usually much more difficult to find an explicit instance of need-reduction. The problem has customarily been resolved by attributing two functions to the unconditioned stimulus—response-production and need-reduction. Thus, in accounting for an instance of salivary conditioning in which food is used as the unconditioned stimulus, Hull assumes that the food plays a double role. It not only serves to elicit the response to be conditioned, but by acting upon the hunger drive serves to provide the essential factor of need-reduction as well.

On the surface this interpretation seems to be fairly straightforward. However, one soon finds that Hull is not quite sure of how food may actually function to "reduce the hunger need" either in this or in any other learning situation. At one point Hull arrives at the conclusion that the delay between the ingestion of food and its absorption "makes it distinctly improbable" that the ingestion of food provides direct need-reduction, and calls the principle of secondary reinforcement to his aid. But since a stimulus only acquires the property of secondary reinforcement as a result of having

been "closely and consistently associated with need diminution" it becomes necessary to introduce a further set of assumptions to bridge the temporal gap between ingestion and absorption. To this end Hull postulates a complex chain of backwardly-directed stimulus-response connections which finally result in the establishment of a connection between food and a hypothetical "fractional goal response." It becomes unnecessary to make such an elaborate set of inferences if stimulus-substitution is accounted for in terms of sensory integration rather than need-reduction. The theory of sensory integration will also take care of all the data subsumed under the heading of secondary reinforcement, *i.e.*, indications that stimuli contiguously related to goal objects may under certain conditions acquire goal properties. But we need not rely on postulate-counting to provide a solution to the problem. There is abundant evidence with which the need-reduction principle, parsimonious or not, is entirely unable to deal.

When salivary conditioning is established with acid instead of with food, it is no longer possible to regard the unconditioned stimulus as a need-reducer. However, it is possible in this case to maintain that need-reduction occurs as a result of the dilution of acid by the flow of saliva. Unfortunately for this interpretation, Finch (13) has demonstrated that salivary conditioning occurs with acid even when the flow of saliva is inhibited during the training series by the injection of atropine. How then is the need aroused by the acid reduced promptly enough to establish a connection between the conditioned stimulus and a salivary response?

When we turn to experiments with shock, with which Hull has dealt most directly, the inadequacy of his position becomes even more apparent. Seeking for need-reduction in these experiments, Hull arrives at the conclusion (supported by Kendler and Underwood) that the shock creates a need which is reduced when the shock terminates. Buzzer and shock to the leg are paired, and the shock produces both leg flexion and a state of need. Then the shock terminates, the need is reduced, and the buzzer-flexion connection is thus established in accordance with Postulate IV. Hull does not distinguish between avoidance situations and those in which the conditioned stimulus is always followed by brief shock whether or not the conditioned response intervenes. Since the reinforcing state of affairs in learning is not necessarily the physical consequence of the act which is reinforced, as Hull tells us (21, pp. 80–81), it apparently does not matter whether the conditioned response terminates the shock or whether the shock *merely terminates*. Shock creates a need which its termination reduces, and any stimulus response conjunction which happens to be in the temporal vicinity of these events is strengthened as a consequence.

As Hilgard has clearly recognized, Hull's conception of the role of shock may account for the selection of *escape* responses, but it cannot deal with the successful development of *avoidance* responses. He writes:

> "If an animal runs down an alley, meets a charged grid, and leaps ahead from it to safety, the leap is associated with need-reduction (pain-alleviation) so that, by strict reinforcement theory, the next time the animal should do what it last did, only perhaps more intensely. It should run down the alley faster, touch the grid and make a jump to safety. . . . Rats do not do this. They slow up in the alley and try not to approach the grid. That is the difference between avoidance and escape behavior" (19, p. 108).

Evidently one can only account for avoidance if one assumes that stimuli contiguously related to the *onset* of shock acquire the functional properties of shock.

When we turn to the data of some representative conditioning studies, we find clear evidence of the inadequacy of Hull's interpretation of the function of shock. First let us consider the experiment by Brogden on the effectiveness of substitute rewards. Brogden (2) found that a conditioned avoidance response, established with the use of shock, could be maintained at a high level by substituting food for shock. Food was given whenever the conditioned response was made and withheld when the response failed to appear. These results establish the fact that shock may play a motivational role in the maintainence of an established conditioned response, and that the hunger motive may effectively replace the shock motive. It should be noted, however, that in this experiment the *absence* of shock rather than the *onset and termination* of shock functioned as the motivational equivalent of food.

Once the feasibility of reward substitution has been established, it becomes possible to evaluate the results of an experiment by Brogden, Lipman, and Culler (7). These investigators studied the rate at which an avoidance response to a tone of 1000 cycles extinguished under two different conditions. In procedure A, reaction to the conditioned stimulus was followed by thoracic shock instead of by the leg shock which had served as the unconditioned stimulus in the original training. In procedure B, the tone was never reinforced in any way; shock was entirely eliminated. A strict application of Hull's interpretation of the role of shock leads to the prediction that extinction should occur more rapidly under condition B, since the termination of thoracic shock which closely follows each tone-flexion conjunction in condition A should constitute a reinforcing state of affairs and thus tend to maintain the response. In fact, Hull's conception requires us to predict that no extinction should occur at all under condition A, since the termination of thoracic shock should provide as much need-reduction, *per se*, as does termination of leg-shock. But these predictions are not at all borne out by the experimental results. Extinction occurred under both conditions, and considerably more rapidly under condition A. Thoracic shock accelerated rather than retarded the extinction process. Certainly shock has been demonstrated to have motivational significance in this experiment, but it functions in the reverse of the manner which Hull describes.

Another experiment by Brogden (4) indicates that once a conditioned avoidance response has been established it *can* be maintained following a shift in the locus to which the unconditioned stimulus is applied, but *only* when the shock is administered for *failure* to make the conditioned response. Under these circumstances the conditioned response is *never* reinforced in Hull's sense of the term; that is, the conditioned response is never followed by shock and shock-termination, while failure to respond is *always* followed by shock and shock-termination. If Hull's interpretation of the role of shock were correct, therefore, we should expect that the conditioned response would soon disappear.

But Brogden's experiment (4) goes even further. It demonstrates that an extinguished avoidance response can be *reestablished* when shock to a new locus is administered for failure to respond. Brogden conditioned four animals to a bell, using shock to the *left* forepaw as the unconditioned stimulus. As soon

as an avoidance response was established, shock was entirely eliminated and the response extinguished. Now the bell was reintroduced, and followed by shock to the *right* forelimb when the animals failed to respond. The shock produced flexion of the right forelimb, but in a few trials the animals began to respond to the conditioned stimulus with flexion of the left forelimb and this response was maintained at a high level. The conditional response reappeared so quickly in this experiment that there was no opportunity for a new sensory integration—bell-shock to right forelimb—to be established. Further work by Brogden indicates, however, that when the conditions were changed in such a way as to require a greater number of contiguous presentations of bell and shock to the right forelimb, a new integration was established and "ambiguous" conditioning resulted.

All of these studies point clearly to the inadequacy of Hull's interpretation of the role of shock in flexion conditioning. A brief shock contiguous with a stimulus-response conjunction does not in itself establish a tendency for the stimulus to elicit the response. And if the Hullian interpretation is rejected, there is no alternative but to postulate a process of sensory integration to account for stimulus substitution. Guthrie's (18) version of the contiguity principle is not an acceptable alternative because the phenomenon of extinction demonstrates that mere contiguity of *stimulus and response* has no reinforcing value.

Before we proceed to the sensory preconditioning studies, it is necessary to deal with another experiment which has been discussed both by Maier and Schneirla and by Kendler and Underwood, and which bears on Hull's conception of the role of shock. Brogden, Lipman, and Culler (7) trained eight guinea pigs to run in a modified activity wheel. A 1000 cycle tone served as the conditioned stimulus and shock as the unconditioned stimulus. For four of the animals, the tone was always followed by brief shock irrespective of the kind of response which appeared. For the other four, running terminated the shock and running to the tone entirely prevented the occurrence of shock. The effect of the two procedures on the incidence of running to the tone was measured.

Unfortunately Brogden, Lipman, and Culler present no data on the initial responses of the animals to the shock. However, one may assume without much danger of error that the initial incidence of running was the same for both groups. Since running to shock was followed in both groups by shock-termination (in one group running terminates the shock while in the other group shock terminates automatically after a brief interval), an equal initial tone-run connection should have been established in each group. If Hull's view is correct once the conditioned running has made its appearance, the performance of the two groups should diverge. In the avoidance group, running to the tone is *never* followed by shock-onset—which means that this behavior is never followed by shock-termination—and consequently is never "reinforced." For the non-avoidance group, on the other hand, the conditioned running response is always followed by brief shock, and consequently by the need-reducing properties of shock-termination. Under these circumstances Hull's theory leads to the prediction that the conditioned running response in the avoidance group should be a relatively weak and unstable affair. Each time the response occurs a process of extinction should automatically ensue. What Brogden, Lipman, and Culler found, however, was that

running rapidly came to be the exclusive mode of response to the tone in the avoidance group. Within an average 200 trials the avoidance animals achieved a perfect score and ran consistently to the tone. On the other hand, despite the fact that running was always followed by shock-termination for the non-avoidance group the incidence of running remained low. For the most part these animals "literally 'sat tight,' held the breath, and tensely awaited the shock" (7, p. 111).

In order to account for these results, Maier and Schneirla (31) find it necessary to propose a two-fold process. In both groups of animals there occurred a process of sensory integration in which the tone, as a result of its contiguous relation to the shock, acquired the functional properties of shock. In addition, there occurred a process of selective modification of response. We do not have full information on the course of response modification in the non-avoidance group, but we are told that the crouching pattern became the predominant response of these animals to the tone-shock combination, perhaps became crouching to some extent diminished the effectiveness of the shock. Nevertheless, the course of selective learning in the avoidance group is clear. The running pattern rapidly became the principal mode of response to the tone *because* it was never followed by shock.[1]

[1] In a recent repetition of this experiment, Sheffield (35) found, as one might expect, that certain signs of extinction *did* appear following a number of successful avoidance responses. "As training continued, however, more and more successive conditioned responses occurred without requiring reinforcement" (p. 171). Sheffield's avoidance animals, unlike those of Brogden, Lipman, and Culler, did not reach the 100 per cent level probably, as he suggests, because they had previously been given many trials of non-avoidance training.

In dealing with this position Kendler and Underwood imply that stimulus substitution and selective learning must be assumed to occur successively in time. However, this assumption is not essential to the theory. Once it is realized, however, that temporal sequence is not a crucial problem, the whole point of the Kendler-Underwood argument disappears. The development of stimulus equivalence and the modification of response are not to be regarded as temporal phases. These two *kinds* of learning represent qualitatively different processes which may occur successively or concurrently, dependently or independently of each other. In the study by Brogden, Lipman and Culler, therefore, two kinds of learning are occurring in both groups of guinea pigs. One kind of learning involves stimulus substitution—the tone becomes the equivalent of the shock. The other kind of learning is selective learning—the response to the unconditioned stimulus (or, after effective equivalence is established, to the conditioned stimulus) is modified to adapt the animal to the motivational requirements of the situation. Both groups of animals may have common associative learning. However, the nature of response modification differs for the two groups. Since the consequences which are attendant upon the responses are different, the animals fixate those contrasting patterns of response which have adaptive value. Considered in this light, contiguity learning and selective learning are occurring simultaneously, with contiguity determining the development of stimulus equivalence and selective learning determining the organization of the response pattern. From this point of view, the variations of this experiment proposed by Kendler and Underwood to test the two-process theory have no relevance whatsoever.

In dealing with the criticisms ad-

vanced by Kendler and Underwood it is also necessary to consider the results of an experiment by Girden (14) which they believe to contradict the position of Maier and Schneirla. Girden presented shock to the foreleg of the dog for varying numbers of trials prior to presenting the stimulus to be conditioned. The nature of the conditioned response obtained depended on the stage of response modification which had been developed in the course of experience with the unconditioned stimulus. As Girden reports: "If the unconditioned response was still at the reflexive stage, then the conditioned response assumed the same form. If, however, sufficient preliminary training with the unconditioned stimulus had been first given, then the voluntary conditioned response appeared in short order" (14, p. 679). These results are directly relevant to the conception of qualitatively distinct processes—stimulus substitution and selective learning —operating in the conditioning situation. The experiment simply reveals that a certain amount of selective learning may occur prior to the process of stimulus substitution.

2. *Sensory preconditioning.* More than twenty years ago, Prokofiev and Zeliony (33) performed the first of a series of experiments which provide strong support for the contiguity principle. These investigators presented their human subjects with the sound of a metronome for 10 seconds and immediately after applied rhythmic pressure to the forearm. This procedure was continued for several days, 5 to 10 times each day. Next the tactile stimulus was paired with an electric shock to the hand until conditioned arm withdrawal was established. When the metronome was subsequently presented alone, it was successful in eliciting arm withdrawal in two of the three subjects. As it stands, this study, although suggestive, does not permit of unambiguous interpretation, because of the small number of subjects and the failure to control possible generalization from auditory to rhythmic tactual stimulation.

Shipley (36) continued the investigation of the problem by using a larger group of subjects and conditioning the eye wink evoked by a sudden tap on the cheek to the weak flash of an electric light. After conditioning had been established, the tap on the cheek was paired with shock to the finger until conditioned finger withdrawal occurred. Now, upon presentation of the light flash alone, nine of the fifteen subjects responded by finger-withdrawal, a response which had never before occurred in conjunction with the light stimulus. In a control group of 10 subjects, tap on the cheek was given alone and then paired with shock to the finger until a conditioned withdrawal was established. A light flash presented to these subjects produced no instance of finger withdrawal, demonstrating that the behavior of the experimental subjects was not the result of stimulus generalization.

Maier and Schneirla believe that Shipley's results are consistent with a sensory integration theory. In the first stage of Shipley's experiment, the light flash becomes equivalent to strike and in the second stage strike becomes equivalent to shock. Presentation of light therefore results in finger-withdrawal. Hull does not deal with the same experiment as do Maier and Schneirla, but with another study by Shipley (37). However, his discussion may be extrapolated to provide an explanation for the experiment which Maier and Schneirla discuss. Hull interprets the finger retraction to light as the result of indirect generalization. Such generalization occurs because the light evokes the wink; the wink produces a proprioceptive stimulation; and

since the proprioceptive stimulation was conditioned to finger retraction in the second phase of Shipley's procedure, it later evokes the withdrawal response.[2] Therefore, the new effectiveness of the light is dependent upon the occurrence of the wink. Fortunately, there is some experimental evidence which bears on the role of the wink-aroused proprioceptive stimuli upon which Hull calls to account for Shipley's results. Lumsdaine's (30) photographs show that on some occasions finger withdrawal occurs prior to the wink or even in the absence of the wink. However, Hull refuses to be tied to any explicit behavior and vaguely speaks of "numerous other reactions conditioned at the same time as the wink" (21, p. 193). This vagueness makes his hypothesis entirely untestable and should lead to its immediate rejection on the basis of the criteria which he himself sets forth.

Several recent investigations of the phenomenon of sensory preconditioning avoid the weakness of earlier work by using stimuli in the early presentations which can only with difficulty be considered to have need-reducing properties. In an experiment conducted with dogs, Brogden (3) subjected one group of animals (experimental group) to 200 simultaneous paired presentations of a bell and a light. A second group of animals (control group) received no such paired stimulation. Subsequently, both the control and experimental animals were trained to make a forelimb flexion response to one of the stimuli. In the case of the experimental animals it was found that when forelimb flexion had been established to one of the stimuli (either light or bell) the presentation of the other stimulus also tended to evoke the flexion response. That this behavior was not simply the result of stimulus generalization is shown by the fact that the control group, which had not experienced the contiguous presentation of the two stimuli earlier, did not respond with forelimb flexion to the stimulus to which the flexion had not been explicitly conditioned.

Brogden further explored the phenomenon of sensory pre-conditioning in two studies at the human level. One of these studies was unsuccessful in demonstrating the phenomenon because of an unfortunate choice of experimental procedure (5). The second experiment (6) shows sensory pre-conditioning at the human level quite clearly. In this study one experimental and several control groups were used. The subjects comprising the experimental group were first placed in a room and observed through a one-way window. When the subjects' gaze happened to be directed toward the stimulus cabinet, a light and a complex tone were simultaneously presented. When a total of 10 such paired stimulus presentations had occurred, the subjects were given instructions leading to the formation of a key-pressing response to light. They were then presented with a sequence of 10 light, 10 tone, and 10 light stimuli. The subjects of one control group (sensory-generalization control) received the same testing but no initial paired presentation of light and sound. Another control group received the same test, but prior stimulation with tone alone. The number of responses to tone (although the subjects had only been instructed to respond to light) was significantly greatest for the experimental group, while no significant

[2] Hull must assume an intermediate response, the wink to the light flash, as the basis for the *new* effectiveness of the light flash. In this interpretation he must also assume that two backward conditionings occur and that strike termination has a need-reducing function. Here we have a plethora of unsupported hypotheses in a presumably parsimonious theory.

differences appeared among the control groups.

A study by Karn (22) on conditioned finger-withdrawal corroborates Brogden's findings. Prior to establishing the finger-withdrawal pattern, Karn stimulated his experimental group (12 subjects) 50 times with simultaneously presented light and sound. The control group (12 subjects) did not have this experience. After a buzzer-shock avoidance response had been established to the criterion of five successive anticipatory finger withdrawals, both groups were stimulated with ten presentations of light alone. The experimental group made 75 withdrawals out of 120 possible responses, whereas the control group made only nine withdrawals out of a possible 120 responses. Further, only two of the experimental subjects failed to respond to light, while eight of the control subjects failed to do so. Karn attributes this difference in behavior "to an association formed between visual and auditory stimuli during the preliminary pre-conditioning period" (22, p. 544).

Sensory pre-conditioning has also been demonstrated at the level of the rat. In an unpublished Cornell experiment, rats were placed in a box, the floor of which was covered by a grid. In the first stage of the experiment light was paired with brief shock from the grid. As soon as the animals showed signs of being excited by the light itself, continuous shock was introduced and the animals learned to escape through a door which had previously been locked. In the third phase of the experiment, light alone elicited the escape response. Stimuli not previously paired with shock did not produce this response.

The results of the sensory pre-conditioning experiments require us to postulate a process of afferent modification (sensory integration), the essential condition for which is contiguity of stimulation, and which takes place independently of need reduction. In the Brogden and Karn studies paired presentation of bell and light establishes a functional relation between them which is demonstrated by the fact that when one of the pair becomes the effective stimulus for a new response the other also becomes capable of eliciting the response. It would be too far-fetched to contend that the bell-light combination constitutes a reinforcing "state of affairs." It might be contended that the termination of shock in the first stage of the Cornell experiment provided the requisite need-reduction, although we have already demonstrated the inadequacy of this conception. Even if this assumption is made, the fact that the light subsequently elicited the directed escape response with which it had never before been in conjunction cannot be explained. Only a concept of sensory integration can deal adequately with these results.

3. *The experimental delimitation of neural structures involved in conditioning.* In defending Hull's need-reduction principle, Kendler and Underwood have maintained that they are not concerned with evidence bearing upon the operation of any specific type of neural mechanism in learning. The theory, they contend, is a "non-reductive" one and has no relation to physiological processes once "language trends" have been "discounted." This assertion is patently contrary to fact. Anyone who has read Hull knows that he makes use of supporting physiological data. But irrespective of Hull's practice, the present writers hold that since the material substratum for learning is the nervous system, a fruitful theory is one that yields inferences of a neurological kind, which in turn yield testable hypotheses crucial for the theory. A non-reductive

approach indicates theoretical weakness rather than theoretical strength.

As we have already indicated, Maier and Schneirla are not content merely to defend the principle of stimulus substitution by contiguity. In order to explain the observed relationships, they advance a conception of sensory integration which is the beginning of a neurophysiological theory. This conception, preliminary as it is, fits the results of a variety of experiments in which attempts have been made to delimit the neural structures involved in conditioning.

Loucks (29) found that when a buzzer was paired with a shock stimulus which was applied directly to the motor cortex and thus elicited leg flexion, no conditioning could be established. However, when the animal was given food each time leg flexion occurred, the buzzer soon began to elicit the flexion response. In the first part of the experiment, say Maier and Schneirla, neither sensory integration nor selective learning could occur. There could be no sensory integration because there was no contiguity of afferent processes, and there could be no selective learning because the flexion response was not rewarded. In the second phase of the experiment, the food reward made selective learning possible. Kendler and Underwood disagree with the interpretation in so far as the first phase of the experiment is concerned. They reject the conception of sensory integration and maintain that the absence of effective sensory relationships was unimportant. Learning failed to occur, they assert, because the response was not followed by need diminution. But what need diminution follows the flexion response when it is aroused, not by cortical shock, but via the afferent pathways of the leg? To answer this question, Kendler and Underwood must attribute need-reducing properties to shock termination, a conception which we have already shown to be inadequate.

Another experiment which bears on this problem is that of Kleitman (26) who attempted to establish conditioned salivation in dogs when the salivary response was produced by pilocarpine, an agent which acts directly to arouse the salivary gland. At no time was conditioned salivation established by such a procedure although the conditioned stimulus was paired with the unconditioned response in hundreds of training trials. However, conditioned salivation could easily be established if the agent which caused the increased activity of the salivary gland was morphine, which acts indirectly through the central nervous system. Finch (12) working some ten years later confirmed Kleitman's findings. Why were the results with morphine different from those obtained with pilocarpine? The concept of sensory integration suggests that it was because the morphine provided effective sensory relations which pilocarpine did not. The Hullian can only assume, *ad hoc*, that the morphine must have provided some sort of need-reduction while the pilocarpine did not.

The concept of sensory integration leads Maier and Schneirla to the prediction that conditioning can take place even when the response to the unconditioned stimulus is prevented from occurring, and the results of a variety of experiments are consonant with this view. Light and Gantt (28), working with dogs, crushed the motor pathways to the right hind leg, thus producing a temporary but total motor paralysis of this limb. During the period of total paralysis the investigators repeatedly paired a buzzer with electric shock to the paralyzed limb. At no time during this training period did the limb exhibit flexion, although other general movements of the body occurred. Af-

ter training, the motor pathways were allowed to regenerate, and tests for conditioning were made. In these tests the buzzer elicited the flexion response which had never before appeared.[3] Crisler (9) and Finch (13) have reported similar results with salivary conditioning. Although during training the glandular response was inhibited by atropine, the conditioned response was manifested after the withdrawal of the inhibitory agent.

In order to deal with results of this sort, Kendler and Underwood find it necessary to abandon their "non-reductive" approach and make hypotheses about physiological processes in the animal. In discussing the Light and Gantt experiment they write:

"A reinforcement theory does not demand, of course, that the response per se has to occur for reinforcement to occur. Since the cessation of the shock produces a reinforcing state of affairs, a functional relation can be established between the conditioned stimulus and the potential overt response (efferent discharge). Light and Gantt point out that it is reasonable to assume that the efferent discharge for foot withdrawal was made regardless of the fact that it did not produce the response while the nerve was crushed" (25, p. 214).

The quotation also shows clearly that the Kendler-Underwood interpretation stands or falls with the conception that termination of shock in itself constitutes a reinforcing state of affairs. Since the inadequacy of this conception has already been demonstrated, it seems necessary to postulate a process of sensory integration in order to account for the data in question.

Maier and Schneirla also suggest that the results obtained in work with decorticate dogs (11, 16) lend support to the distinction between conditioning (sensory integration) and selective learning. In these experiments shock to the limb was employed as the unconditioned stimulus and a "diffuse" or "massive" type of conditioned response readily appeared. A comparison of the performance of decorticate and intact animals in the same situation revealed that while in the intact specimen the initial diffuse response to the conditioned stimulus is modified and converted into a precise response, the decorticate animal is limited to the initial diffuse reaction. According to Maier and Schneirla, "this seems to mean that association by contiguity is more primitive than selective learning, since it can proceed in the absence of cerebral cortex although selective learning is then excluded. . . . The postulation of two qualitatively different learning mechanisms makes understandable the fact

[3] These findings were confirmed in a repetition of the experiment by Kellogg, Scott, Davis and Wolf (24), although they offer a somewhat different interpretation. They conclude that "the conditioned flexion response is really a general reaction involving postural and other bodily changes. To eliminate one segment of the response does not eliminate conditioning. When not prevented from doing so, the segment fits readily into the whole of which it is a natural part" (p. 72). We are also told, however, that "the paralyzed subjects . . . (by swaying, swinging, or stretching) . . . were actually accomplishing the result of lifting the foot, but without the use of the flexing muscles, during their training. With the return of function in the previously paralyzed members, they substituted the direct for the indirect method of accomplishing the same result" (p. 73). Evidently the general pattern which appeared after recovery was not the pattern elicited by the unconditioned stimulus during the training trials.

Recently Girden (15) has presented new evidence in support of a motor theory of conditioning. The evidence which he presents is unconvincing, however, since it is based on work with erythrodine, a curare-like substance which he himself has demonstrated to have widespread cortical effects that are not very well understood at the present time. Girden's argument rests on the assumption that the two drug states employed in his experiments are "continuous" for skeletal processes because they appear to be continuous for visceral processes.

that the first stage in conditioning can occur unimpaired although the second is excluded in spinal and decorticate animals" (31, p. 122). Kendler and Underwood suggest, on the other hand, that the results in question can be accounted for on the assumption that the decorticate dog is "incapable of developing" an organized response to the shock. But this is merely a restatement of the problem. Why is the decorticate dog incapable of developing an organized response to shock? If stimulus substitution and selective learning are not distinct processes, why is one possible and not the other? Certainly, as Bromiley (8) has demonstrated, the decorticate dog is capable of specific response to shock, and when the shock is such as to elicit a specific response, the conditioned stimulus with which it is paired elicits the same response. Apparently it is selective modification of response which cannot occur in the decorticate animal.[4]

SENSORY INTEGRATION AS A GENERAL PROCESS OF LEARNING

Thus far we have attempted to demonstrate that the development of stimulus equivalence in conditioning experiments requires us to postulate a process of sensory integration—a process of afferent modification which depends primarily upon contiguity of stimulation and which can occur in the absence of need-reduction. We may now ask whether the applicability of this conception is limited to the kind of data with which it was derived to deal, or whether its usefulness is not considerably greater. Although the process of sensory integration is most clearly revealed in conditioning, we believe that it will also make it possible to explain many of the phenomena which occur in a variety of other learning situations. We do not think that all phases of maze, discrimination, or problem-box learning can be reduced to a process of afferent modification, but once we are able to determine those aspects of learning which can, and those which cannot, be accounted for in terms of sensory integration, we will be in a better position to postulate complementary processes.

In the previous section we have spoken of sensory integration as though it occurs only as a result of the contiguous presentation of exteroceptive stimuli, under the full control of the experimenter. It is now possible to introduce a broader conception of the conditions under which the process occurs. Consider, for example, the case of a hungry rat which approaches the positive card in a discrimination situation and finds food behind it. On the next trial the animal will be more likely to approach the positive card, but it is not necessary to conclude that a direct functional relation (or connection) between the card and the adient response has been established. Thinking in terms of sensory integration, we may say that the positive card has acquired some of the functional properties of the food with which it has been in contiguous relation, so that adient responses formerly elicited by the food alone will now be elicited by the card.

Much of the same sort of explana-

[4] Moreover, the conditioned responses developed by the operated animal often seem to have need-increasing, rather than need-reducing consequences. For example, when the animal in the experiment of Girden *et al.* (16) was about three-quarters decorticated, its response to the shock to the foreleg consisted of bilateral foreleg extension, a response which placed the feet more firmly against the grill and thus intensified the direct effectiveness of the shock. This response was readily conditioned to the auditory stimulus. The writers say that "the new conditioned response was obviously a perfect case of diffuse behavior which is decidedly non-adaptive; since at every trial the dog, by planting the forefeet on the grids, received the full shock instead of escaping it" (p. 372).

tion may be suggested for the elimination of blind alleys in the maze. In preliminary trials, it may be assumed, a cul entrance acquires the functional properties of the cul's end by a process of sensory integration which, depending on the length and complexity of the blind alley, may occur directly or in stepwise fashion. This conception of progressive afferent integration is consonant with the facts of mammalian neural organization and provides a process-oriented approach to phenomena with which the cognitive theorists (27, 40) have been able to deal only in dangerously anthropomorphic terms, and with which Hull has been entirely unable to deal. The fact that sensory integration may proceed in the absence of need and need-reduction suggests an explanation of "latent learning" which does not require us to assume that the rat can function at the intellectual level of man. At the present time we know very little about the conditions under which sensory integration occurs, although it is probable that the powerful afferent consequences of drive states which exist at any given moment may play a significant role in the process. The concept of sensory integration is not so much a solution of problems as it is a starting point for a whole series of new investigations designed to explore the process of perceptual organization at different developmental levels.

The concept of sensory integration may even enable us to deal with some aspects of modification of behavior in the problem box, which has long been dominated by the effect principle. In the Skinner box, for example, the rat experiences food only in the vicinity of the lever. As the lever and other local stimuli acquire food-properties, the animal orients to these stimuli and the only responses which appear are those which they elicit. The critical features of the stimulus situation may in this way be differentiated out of the mass, and the range of behavior accordingly restricted.

It is unlikely, however, that response modification can be fully accounted for in afferent terms. Many of these changes in behavior apparently require the direct reorganization of afferent-efferent relationships. Since there is no reason to believe that proprioceptive stimuli cannot participate in the integration process, we may assume that the specific proprioceptive consequences of the correct response may also come to acquire food properties (17). But how this afferent modification results in the dominance of the correct response is another question. The problem posed by results such as those of Loucks (29) illustrates the limited applicability of the sensory integration concept. Loucks paired a buzzer with shock to the motor cortex of the dog. The shock produced flexion, and if the flexion was followed by food, the buzzer alone soon began to elicit the response. The buzzer may have been integrated with flexion afferents and with food afferents, or with both, but since neither of these components evokes flexion, the integration does not explain why the buzzer acquired that property.

The cognitive theorists do not face this problem. How "knowledge of means-end-relations" is translated into appropriate behavior—how afferent integrations are translated into appropriate efferent patterns—we are not told. Nevertheless, the establishment and alteration of functional relations between afferent and efferent processes present a theoretical problem which must sooner or later be solved. Effect theory represents an attempt to define the conditions under which changes of this kind take place, but it gives no hint of the underlying mechanisms.

SUMMARY

The classical investigations of Pavlov and Thorndike suggest the operation of two qualitatively distinct learning processes. The conditioning situation directs attention to the development of stimulus equivalence (stimulus substitution—a process of afferent modification (sensory integration) which results from contiguity of stimulation and which may occur in the absence of need-reduction. The problem box emphasizes the selective modification of response in accordance with motivational requirements.

The contemporary tendency to break down the distinction between these processes, as it is exemplified in the system of Hull, is here evaluated. Hull and his adherents have attempted to reduce all learning to the establishment and strengthening of stimulus-response connections under the influence of need-reduction. The stimulus substitution which occurs in conditioning experiments can be accounted for in these terms only if it is possible to demonstrate that the process depends upon (a) the close conjunction of the conditioned stimulus and the response which it is to elicit and (b) contiguous need-reduction. Our analysis of the evidence reveals that stimulus equivalence develops even when neither of these conditions is met. We conclude, therefore, that at least *two* processes of learning—sensory integration and selective modification of response—must be postulated if the data of conditioning experiments are properly to be understood. Neither of these processes is unique to conditioning, but both may operate in a variety of learning situations.

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[MS. received March 28, 1949]

Errata

In C. J. Burke's article "A Theory Relating Momentary Effective Reaction Potential to Response Latency" (PSYCHOL. REV., 1949, **56**, 208–223) equations (1′) and (1) are incorrect and should read as follows:

$$\bar{E} = E_0 + (M_E - E_0)(1 - e^{-iN}) = f_1(N).$$

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